PROCEEDINGS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA

FOR

PROMOTING USEFUL KNOWLEDGE.

Vol. XXXV.

JANUARY TO DECEMBER, 1896.

PHILADELPHIA:
THE AMERICAN PHILOSOPHICAL SOCIETY,
1896.

Gel

7 418

506 A6
v.35

PROCEEDINGS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA FOR PROMOTING USEFUL KNOWLEDGE.

VOL. XXXV. JANUARY, 1896. No. 150.

Stated Meeting, January 3, 1896.

Mr. W. V. McKEAN in the Chair.

Present, 27 members.

Reports of the Clerks and Judges of the election were read and the report of the election was submitted:

President.

Frederick Fraley.

Vice-Presidents.

E. Otis Kendall, J. P. Lesley, Wm. Pepper.

Secretaries.

George F. Barker, George H. Horn, Persifor Frazer, Patterson Du Bois.

Curators.

J. Cheston Morris, R. Meade Bache, Benj. S. Lyman.

Treasurer.

J. Sergeant Price.

PROC. AMER. PHILOS. SOC. XXXV. 150. A. PRINTED APRIL 13, 1896.

Councilors.

Wm. A. Ingham, Chas. S. Wurts, Robert Patterson, Henry Hartshorne, Isaac J. Wistar, in place of Richard Vaux, deceased.

On motion, the thanks of the Society were tendered to Judge Edmunds and his associates for their services on the board of election.

Letters of envoy from the Geological Survey of India, Calcutta; Naturwissenschaftlichem Vereine, Osnabrück, Prussia; Society of Antiquaries, London, Eng.; Direccion General de Estadistica, Mexico, Mex.

Letters of acknowledgment from the Linnean Society of N. S. Wales, Sydney (143, 146); Geological Survey of India, Calcutta (147); M. G. Tschermak, Vienna, Austria (147); Naturwissenschaftl. Gesellschaft "Isis," Dresden, Saxony (147); Società Italiana d'Igiene, Milan, Italy (143, 146); Società Africana d'Italia, Naples, Italy (147); R. Accademia di Scienze, Lettere, etc., Padua, Italy (143); R. Comitato Geologico d'Italia, Rome (147); Dr. Charles S. Wurts, Philadelphia (147); California Academy of Sciences, San Francisco (144, 145).

Accessions to the Library were reported from the Schweiz. Naturfor. Gesellschaft, Schaffhausen; Thüringische Geschichte und Altertumskunde, Jena, Germany; Mr. Horatio Hale, Ottowa, Canada; Dr. Samuel A. Green, Boston, Mass.; Dr. C. A. M. Fennell, Cambridge, Mass.; Prof. E. J. James, Mr. B. S. Lyman, Philadelphia; U. S. National Museum, U. S. Dept. of Agriculture, Washington, D.C.; Academy of Sciences, Department of Public Works, Chicago, Ill.; Agricultural Experiment Station, Ames, Ia.; Dr. Jesus Diaz de Leon, Aguascalientes, Mexico; Sociedad Cientifica, "Antonio Alzate," Mexico, Mex.

The stated business of the meeting being the nomination of Librarian, J. Sergeant Price nominated George H. Horn, and E. D. Cope nominated Benj. S. Lyman.

Prof. Cope made a verbal communication on certain

types of Saurians in completion of a former paper on the same subject.

Pending nominations 1332 to 1334 were read.

Mr. Wm. A. Ingham moved to amend Chapter viii, Section 3 of the Laws, by striking out "from 10 A.M. to 1 P.M.," and inserting " at such hours as the Society may by resolution from time to time direct." Laid over under the laws.

The Judges of election reported the receipt of a paper questioning the eligibility of a candidate. As they deemed the question beyond their jurisdiction the paper was referred to the Society for action. On motion, the President was requested to appoint a committee of three to investigate and report upon it.

After reading the rough minutes, the Society was adjourned by the presiding member.

Stated Meeting, January 17, 1896.

President, Mr. FRALEY, in the Chair.

Present, 36 members.

Correspondence was submitted as follows:

Invitation from the Société Impériale Russe de Geographie, St. Petersburg, to attend the Fiftieth Anniversary of its foundation, February 2 (January 21), 1896.

Letter from Mr. Thomas Meehan, offering to take in hand the labeling of the South American plants from Dr. Barton's collection belonging to the Society, and suggesting that they be deposited in the herbarium of the Academy of Natural Sciences, Philadelphia.

A communication from the Librarian of the University of Virginia, Charlottesville, stating that their complete set of the A. P. S. *Proceedings* was destroyed by fire, October 27, 1895, was referred to Secretaries with power to act.

Letters of acknowledgment (148) were received from the Wag-

ner Free Institute, Franklin Institute, Historical Society of Pennsylvania, College of Physicians, Numismatic and Antiquarian Society, Gen. I. J. Wistar, Hon. Mayer Sulzberger, Profs. John Ashhurst, Jr., F. A. Genth, Jr., H. D. Gregory, James MacAlister, James Tyson, M.D., Drs. John H. Brinton, W. C. Cattell, Samuel G. Dixon, Ed. A. Foggo, George H. Horn, Morris Longstreth, Charles A. Oliver, Charles Schäffer, D. K. Tuttle, William H. Wahl, Messrs. R. Meade Bache, Henry C. Baird, George Tucker Bispham, Lorin Blodget, Arthur E. Brown, Jacob B. Eckfeldt, Benjamin Smith Lyman, Theodore D. Rand, J. G. Rosengarten, F. D. Stone, Philadelphia; Mr. Heber S. Thompson, Pottsville, Pa.; Dr. W. H. Appleton, Swarthmore, Pa.; Dr. John Curwen, Warren, Pa.; Prof. J. T. Rothrock, West Chester, Pa.; Prof. Ira Remsen, Baltimore, Md.; University of Virginia, Prof. J. W. Mallet, M.D., Charlottesville.

Accessions to the Library were reported from the Institut Egyptien, Cairo; Académie Imp. des Sciences, St. Petersburg, Russia; Friesch Genootschap van Geschied, etc., Leuwarden, Netherlands; Académie des Sciences, Cracow, Austria; K. K. Geologische Reichsanstalt, Vienna, Austria; Gesellschaft für Anthropologie, Ethnologie, etc., Berlin, Prussia; Gartenbauverein, Darmstadt, Germany; K. Sächs. Gesellschaft der Wissenschaften, Leipzig; Nassauischen Vereine für Naturkunde, Wiesbaden, Prussia; Biblioteca N. C., Firenze, Italia; Société de Geographie, Lille, France; Redaction *Cosmos*, Le Mqs. de Nadaillac, Paris, France; Meteorological Office, R. Geographical Society, Editors of *Nature*, R. Microscopical Society, Editors of *The Geological Magazine*, London, Eng.; Agricultural Experiment Station, Durham, N. H.; Mass. Historical Society, Boston, Mass.; Astronomical Observatory of Harvard College, Cambridge, Mass.; Essex Institute, Salem, Mass.; R. I. Historical Society, Providence; Editors of *The American Journal of Science*, Yale University, New Haven, Conn.; Editor of *The Popular Science Monthly*, Academy of Sciences, Editor of *The World*, New York, N. Y.; College of Pharmacy, Franklin Institute, Amer. Society for the Extension of University

Teaching, Mr. Wharton Barker, Drs. Persifor Frazer, Edmund J. James, Hon. Samuel W. Pennypacker, Philadelphia; Oberlin College Library, Oberlin, O.

A photograph for the Society's Album was received from Mr. Thomas Clarke, New York, N. Y.

The death of Mr. Henry Hazlehurst, of Philadelphia, on January 11, 1896, æt. 49, was announced, and the President was requested to appoint a member to prepare an obituary notice.

Prof. Cope read an obituary notice of Prof. John A. Ryder.

The stated business of the meeting being the choosing of a Librarian, a ballot was taken, and on count of the vote the Tellers announced the election of George H. Horn.

The choosing of Standing Committees being in order, Mr. Prime moved that they be appointed by the President. Carried unanimously.

The Special Committee appointed to inquire into the eligibility of candidates and electors at the late election made a report which was received and the Committee discharged.

Prof. Hilprecht presented his paper on "Old Babylonian Inscriptions," which on motion was referred to a Special Committee to examine and report. The President appointed Talcott Williams, Patterson DuBois and J. Sergeant Price, the Committee.

Pending nominations Nos. 1332 to 1334 and new nominations Nos. 1335 to 1345 were read.

On motion of Dr. Morris, the nominations of non-residents were referred to Council.

On motion of Mr. Price, the Society authorized the Treasurer to receive payment for loan of the city of Philadelphia now due and payable.

The letter of Mr. Meehan regarding South American plants in our cabinet was referred to the Curators to report at the next meeting.

On motion of Dr. Frazer, amended by Dr. Brinton, the Secretaries were directed to print a revised list of surviving members with their addresses.

The amendment to the Laws offered at last meeting was considered. The Librarian gave proof of advertisement.

By the requisite vote, Chap. viii, Sec. 3, was amended by striking out "10 A.M. to 1 P.M.," and inserting "at such hours as the Society may fix by resolution from time to time."

The rough minutes were then read, and the Society was adjourned by the President.

Stated Meeting, February 7, 1896.

Present, 16 members.

President, Mr. FRALEY, in the Chair.

Correspondence was submitted as follows:

A letter from Prof. George H. Smith, Los Angeles, Cal., expressing grateful appreciation of the honor conferred upon him by the Society, in publishing his essay, and presenting him with two hundred and fifty copies.

A letter from Mr. Benjamin Sharp, Philadelphia, requesting for the Academy of Natural Sciences the privilege of having a microscopical examination made of two pieces of Jade, deposited in the Museum of the Academy. On motion, this request was referred to the Curators.

Acknowledgments were received from the Institut Egyptien, Cairo (147); K. K. Bergakademie, Leoben, Austria (92–107, 110, 111, 113–120, 125–133, 135–147); K. K. Zoolog-botanische Gesellschaft, Vienna, Austria (147); Schlesische Gesellschaft für vaterl. Cultur, Breslau, Prussia (147); Naturhistorische Gesellschaft, Hannover, Prussia (147); Naturhistorische Gesellschaft, Nürnberg, Bavaria (147); Verein für Naturkunde, Offenbach-a.-M., Germany (143, 146, 147); K. Geodätisches Institut, Potsdam, Prussia (147); Biblioteca N. C., Firenze, Italia (147); R. Istituto di Scienze, etc., Milan, Italy (143, 146); Observatorio di Torino, Torino, Italia (147, and *Trans.*, xviii, 2); Prof. G. Sergi, Rome, Italy (147); Commission des Annales des Mines

(147 and *Trans.*, xviii, 2); Musée Guimet, Paris, France (147); Literary and Philosophical Society, Liverpool, Eng. (147, and *Trans.*, xviii, 2); Mass. Historical Society, Boston, Mass. (147); Prof. Edward S. Morse, Salem, Mass. (147); Newberry Library, Chicago, Ill. (147); Museo Nacional, Buenos Ayres, Argentine Republic (147).

Acknowledgments (148) were received from Dr. Alfred R. C. Selwyn, Geological Survey, Ottawa, Canada; Laval University, Hon. J. M. Le Moine, Quebec, Canada; Bowdoin College Library, Brunswick, Me.; N. H. Historical Society, Concord; Boston Society of Natural History, Mass.; Historical Society, Boston Athenæum, Mass. Institute of Technology, State Library of Massachusetts, Dr. Samuel A. Green, Boston, Mass.; Museum of Comparative Zoloögy, Profs. Alexander Agassiz, N. W. Goodwin, F. W. Putnam, Dr. Justin Winsor, Mr. Robert N. Toppan, Cambridge, Mass.; Essex Institute, Prof. Edward S. Morse, Salem, Mass.; Prof. Elihu Thomson, Swampscot, Mass.; Marine Biological Laboratory, Woods Holl, Mass.; American Antiquarian Society, Worcester, Mass.; Brown University, Providence Franklin Society, R. I. Historical Society, Providence; Mr. George F. Dunning, Farmington, Conn.; Conn. Historical Society, Hartford; Prof. H. A. Newton, New Haven, Conn.; Prof. James Hall, Albany, N. Y.; Society of Natural Science, Buffalo, N. Y.; Prof. Edward North, Clinton, N. Y.; Profs. J. M. Hart, W. T. Hewett, Ithaca, N. Y.; Historical Society, N. Y. Hospital, Columbia College, N. Y. Academy of Medicine, N. Y. Academy of Science, Astor Library, Amer. Museum of Natural History, Hon. James C. Carter, Messrs. Thos. C. Clarke, James Douglas, Profs. Isaac H. Hall, J. J. Stevenson, New York, N. Y.; Vassar Brothers' Institute, Poughkeepsie, N. Y.; Geological Society of America, Rochester, N. Y.; Prof. W. Le Conte Stevens, Troy, N. Y.; Oneida Historical Society, Utica, N. Y.; U. S. Military Academy, West Point, N. Y.; Free Public Library, Jersey City, N. J.; Prof. Robert W. Rogers, Madison, N. J.; Natural History Society, Trenton, N. J.; Dr. R. H. Alison, Ardmore, Pa.;

Prof. Martin H. Boyé, Coopersburg, Pa.; Amer. Academy of Medicine, Dr. Traill Green, Prof. J. W. Moore, Rev. Thos. C. Porter, Easton, Pa.; Mr. John Fulton, Johnstown, Pa.; Prof. L. B. Hall, Haverford, Pa.; Linnean Society, Lancaster, Pa.; Rev. J. W. Robins, Merion, Pa.; Library Company, Prof. H. V. Hilprecht, Drs. Edward Foggo, Persifor Frazer, Sara Y. Stevenson, Messrs. H. Clay Trumbull, Joel Cook, Patterson Du Bois, Robert Patterson, Benjamin Sharp, Charles Stewart Wurts, Ellis Yarnall, Philadelphia, Pa.; Rev. F. A. Muhlenberg, Reading, Pa.; Mr. Thos. S. Blair, Tyrone, Pa.; Mr. Philip P. Sharples, West Chester, Pa.; Wyoming Historical Society, Wilkesbarre, Pa.; Col. Henry A. Du Pont, Winterthur, Del.; Maryland Institute, Baltimore, Md.; Mr. T. L. Patterson, Cumberland, Md.; U. S. Artillery Staff, Fort Monroe, Va.; Hon. J. R. Tucker, Lexington, Va.; Mr. Jed. Hotchkiss, Staunton, Va.; Ga. Historical Society, Savannah; Cincinnati Observatory, Cincinnati, O.; Ohio State Archæological and Historical Society, Columbus, O.; Editors of *Journal of Comparative Neurology*, Granville, O.; Oberlin College, Oberlin, O.; Prof. J. L. Campbell, Crawfordsville, Ind.; University of Illinois, Champaign, Ill.; Field Columbian Museum, Newberry Library, Chicago, Ill.; Geological Survey of Missouri, Jefferson City, Mo.; State Historical Society of Wisconsin, University of Wisconsin, Madison; Ia. Masonic Library, Cedar Rapids; Academy of Natural Sciences, Davenport, Ia.; State Historical Society, Iowa City, Ia.; Editor of the *Kansas University Quarterly*, Lawrence, Kans.; Academy of Science, Washburn College, Topeka, Kans.; University of California, Prof. Joseph Le Conte, Berkeley, Cal.; Lick Observatory, Mt. Hamilton, Cal.; State Mining Bureau, Cal. Historical Society, Prof. George Davidson, San Francisco, Cal.; Prof. J. C. Branner, Stamford University, Cal.; Agricultural Experiment Stations, Kingston, R. I.; Storrs, Conn.; Experiment, Ga.; Knoxville, Tenn.; Agricultural College, Mich.; Manhattan, Kans.; Lincoln, Neb.; Fort Collins, Colo.; Tucson, Arizona.

Accessions to the Library were reported from the Asiatic Society of Japan, Yokohama; Société Physico-Mathématique, Kasan, Russia; Société Imp. des Naturalistes, Moscow, Russia; Société des Naturalistes de la Nouvelle Russie, Odessa; Comité Géologique, Imp. Russian Geographical Society, St. Petersburg; Statistika Central Byråns, Stockholm, Sweden; Société R. des Antiquaires du Nord, Copenhagen, Denmark; Académie R. des Sciences, etc., Etat Indépendant du Congo, Bruxelles, Belgique; Société Hongroise de Géographie, Budapest; K. K. Zool.-botanische Gesellschaft, Oesterreichische Touristen-Club, Vienna; Physiologische Gesellschaft, Verein zur Beförderung des Gartenbaues, Berlin, Prussia; K. Sächs. Meteorologisches Institut, Chemnitz; Naturforschende Gesellschaft, Emden, Prussia; Naturforschende Gesellschaft, Freiburg-i.-B., Baden; Deutsche Seewarte, Hamburg, Germany; M. Henri de Saussure, Geneva, Switzerland; Société des Sciences Phys. et Naturelles, Bordeaux, France; Société Historique, Litteraire etc., du Clur, Bourges, France; Société N. des Sciences Nat. et Mathématiques, Cherbourg, France; Société de Borda, Dax, France,; Union Geographique du Nord de la France, Douai, France; Société des Sciences Nat. et Archæologique de la Creuse, Guéret, France; Société Languedocienne de Geographie, Montpellier, France; Sociétés Géologique de France, de Géographie, de l'Enseignement, de Physique, d'Anthropologie, Musée Guimet, Directeur de la Rédaction *Melusine*, Museum d'Histoire Naturelle, Ministre des Travaux Publics, Paris, France; M. Ed. Piette, Rumigny, France; Société des Antiquaires de la Morinie, St. Omer, France; Société de Géographie, Toulouse, France; R. Academia de la Historia, Madrid, Spain; Philological Society, Cambridge, Mass.; R. Astronomical Society, Royal Society, London, Eng.; Geological Society, Manchester, Eng.; R. Society of Antiquaries of Ireland; Commissioner of Public Records, Chief of Bureau of Statistics of Labor, Mass. Institute of Technology, Boston, Mass.; Museum of Comp. Zoölogy, Harvard University, Cambridge, Mass.; Travelers'

PROC. AMER. PHILOS. SOC. XXXV. 150. B. PRINTED APRIL 13, 1896.

Insurance Co., Hartford, Conn.; Meteorological Observatory, Amer. Mathematical Society, Amer. Institute of Electrical Engineers, Amer. Geographical Society, New York, N. Y.; Mr. R. P. Potts, Camden, N. J.; Free Public Library, Jersey City, N. J.; Penna. Board of Charities and Committee on Lunacy, Harrisburg; Engineers' Club, Maritime Exchange, Hon. G. F. Edmunds, Dr. Walter M. James, Philadelphia; Johns Hopkins University, Editors of *Chemical Journal* and *American Journal of Philology*, Baltimore, Md.; Anthropological Society, Smithsonian Institution, Departments of the Interior and State, Washington, D. C.; Academy of Science, St. Louis, Mo.; Editors of *Journal of Comparative Neurology*, Granville, O.; College Library, Oberlin, O.; State Board of Health, Nashville, Tenn.; University of California, Berkeley; Field Columbian Museum, Academy of Sciences, Historical Society, Chicago, Ill.; Geological and Natural History Survey of Minnesota, St. Paul; Agricultural Experiment Stations, Amherst, Mass.; Ithaca, N. Y.; State College, Pa.; Newark, N. J.; Lafayette, Ind.; Minneapolis, Minn.; Observatorio Meteorl. Central, Observatorio Astron. N. de Tacubaya, Mexico, Mex.; Observatorio Meteorl. Central del estado de Veracruz Llave, Xalapa, Mex.

A photograph for the Society's Album was received from Dr. Edward A. Foggo, Philadelphia.

Mr. Sachse presented two pictures. The one is a copy of a pencil sketch of the Hall of the Philadelphia Academy, in which this Society held its meetings for many years, drawn to scale by Pierre E. du Simitière. The other picture is a print published in 1790 of the present Hall of the Society as it was at that time.

On motion, the thanks of the Society were voted to Mr. Sachse for his gift. The Special Committee on Prof. Hilprecht's Paper on Cuneiform Inscriptions reported favorably, and the Publication Committee also reported recommending its publication. On motion, the paper was ordered to be published.

On behalf of the Curators, Dr. Morris reported that they

had met and considered the letter of Mr. Meehan with reference to the botanical collections of Lewis and Clark, and other collections such as those of Muhlenberg, Burton, Bettors and Short, now in the museum of the Society, and had passed a resolution recommending their deposit on the usual conditions with the Academy of Natural Sciences; but after further examination of them Mr. Meehan had written, stating that unless they could be given to the Academy they had better remain where they now are, and expressing a desire that the Society should at some future time establish a herbarium of its own, to which he thought the Academy of Natural Sciences would gladly contribute some of its duplicates.

Dr. Morris moved the discharge of the Curators from further consideration of the matter at present.

The death was announced of the Rev. William H. Furness, D.D., on January 30, 1896, æt. 93; and the President was requested to appoint a member to prepare an obituary notice.

The President announced that he had appointed Dr. Brinton to prepare the obituary of Henry Hazlehurst, and F. D. Stone that of William John Potts, and that the appointments had been accepted.

Prof. Cope made a communication illustrating by blackboard sketches the structure of heads of certain Cetaceans.

Pending nominations 1332 to 1342 and 1344 and 1345 were read.

Dr. Brinton asked the decision of the Chair as to whether any action could be taken on the report of the Special Committee which was read at the last meeting.

The President decided that the matter was finally concluded.

Dr. Green moved that the report of the Committee be entered in full on the minutes. Adopted.

There being no further business, the rough minutes were read and the Society adjourned by the President.

Stated Meeting, February 21, 1896.

Present, 51 members.

The President, Mr. FRALEY, in the Chair.

After the meeting had been called to order, Dr. Frazer moved that the regular order of business be suspended until after the demonstration of the Röntgen ray, and that the President be authorized at his discretion at the close of the discussion to declare the meeting adjourned until February 28.

The motion having been carried by the requisite affirmative vote, Dr. Goodspeed was given the floor, and presented the entire subject in detail.

Prof. Houston followed with a discussion of the subject from its electrical side.

Mr. Sachse followed in its photographic relations, and gave his experiences with different styles of plates.

Prof. Robb stated that he had in his laboratory repeated the Röntgen experiments and found that a Crooke's tube was not essential.

Mr. Carbutt gave his experience in the manufacture of plates, dwelling on the probable utility of those with a celluloid basis.

Mr. Jos. Wharton exhibited a tube containing argon, and showed its action under the induction current.

Dr. Pepper exhibited photographs from Prof. John Cox, of McGill University, one of which illustrated the method of obtaining a confirmation of the suspected position of a bullet between the tibia and fibula.

The papers of these speakers will be found in the *Proceedings*, *in extenso*, together with the discussion which ensued.

Dr. Frazer moved the thanks of the Society to the Electric Storage Battery Renting Co. for their loan of a storage battery for the present demonstration.

At 10.30 the President declared the meeting adjourned until Friday, February 28.

Adjourned Meeting, February 28, 1896.

The Vice-President, Dr. PEPPER, in the Chair.

Letters of envoy from the Verein für Schlesische Insektenkunde, Breslau, Prussia; K. Sächsische Gesellschaft der Wissenschaften, Leipzig; Société des Sciences Physiques et Naturelles, Bordeaux, France; Université de Lyon, France; Faculté des Sciences, Marseilles, France; Meteorological Office, British Association for Advancement of Science, R. Statistical Society, London, Eng.; Field Columbian Museum, Chicago, Ill.; State Librarian, Washington, D. C.

Letters of acknowledgment were received from the South African Philosophical Society, Cape Town (143, 146, 147); Royal Society of Victoria (143, 146); Royal Mint, Melbourne, Australia (143, 146); K. Norske Videnskabers Selskab, Throndhjein, Norway (147); Royal Society of Sciences, Upsal, Sweden (143, 146, 147, and *Trans.*, xviii, 2); R. Danske Videnskabernes Selskab, Copenhagen (147, and *Trans.*, xviii, 2); Verein der Freunde der Naturgeschichte, Mecklenburg, Germany (147); R. Istituto Lombardo di Scienze e Lettere, Milan, Italy (147); Accademia R. delle Scienze, Torino, Italia (147); Académie des Sciences et Belles-Lettres, Angers, France (143, 147); Société N. des Sciences Nat. et Mathématiques, Cherbourg, France (143, 144, 146); Université de Lyon, France (147); Nova Scotia Society of Natural Science, Halifax (148); Public Library, Boston, Mass. (148); Harvard University, Cambridge, Mass. (148); Free Public Library, New Bedford, Mass. (148); Mercantile Library (148), Mrs. Helen Abbott Michael (148), Mr. William A. Ingham (147, 148), Hon. James T. Mitchell, Dr. C. N. Peirce, Messrs. Coleman Sellers, Samuel Wagner, Philadelphia (148); Lackawa. Institute of History and Science, Scranton, Pa. (148); Maryland Historical Society, Baltimore (148); Smithsonian Institution (452 packages), U. S. Geological Survey (147, 148, and *Trans.*, xviii, 2), U. S. Naval Observatory, U. S. Patent Office, Surgeon-General's Office,

Coast and Geodetic Survey, U. S. Weather Bureau, Dr. W. J. Hoffman, Rt. Rev. John J. Keane, Prof. Charles A. Schott, Washington, D. C. (148); Prof. E. W. Claypole, Akron, O. (147); Prof. G. W. Hough, Evanston, Ill. (148); Editor of *Kansas University Quarterly*, Lawrence (145, 146, 147); University of Wyoming, Laramie (148); Agricultural Experiment Stations, Raleigh, N. C. (148); Auburn, Ala. (148); Agricultural College, Michigan (145); Observatorio Astronomico de Tacubaya, Mexico (148); Mariano Barcena, Mexico, Mex. (148); Observatorio Meteorologico Central, Xalapa, Mexico (148).

Accessions to the Library were reported from the Royal Society of S. Australia, Adelaide; Académie R. des Sciences, etc., Copenhagen, Denmark; Nederland, Maatschappij bevordering Nijverheid, Amsterdam; Geschichtsvereins, Aachen, Prussia; Naturhist. Verein der Preuss. Rheinlande, etc., Niederrhein. Gesellschaft, Bonn, Prussia; Verein für Schlesische Insektenkunde, Breslau, Prussia; Oberlausitz. Gesellschaft der Wissenschaften, Görlitz, Prussia; Mr. Augustus R. Grote, Hildesheim, Prussia; K. Sächsische Gesellschaft der Wissenschaften, Leipzig; Deutsche Gesellschaft für Anthropologie, etc., Munich, Bavaria; R. Accademia di Scienze, etc., Modena, Italy; Società R. di Napoli, Italia; R. Accademia dei Lincei, Rome, Italy; R. Accademia delle Scienze, R. Osservatorio, Turin, Italy; Université de Lyon, France; Faculté des Sciences, Marseilles, France; Prof. Gabriel de Mortillet, St. Germain-en-Laye, France; R. Statistical Society, British Association for Advancement of Science, Society of Arts, Zoölogical Society, Meteorological Council, Society of Antiquaries, London, Eng.; Royal Geological Society of Cornwall, Penzance, Eng.; Nat. History and Philosophical Society, Belfast, Ireland; Royal Society of Edinburgh, Scotland; Philosophical Society, Glasgow, Scotland; Geological Survey of Canada, Ottawa; Athenæum, Boston, Mass.; Public Library, Salem, Mass.; Amer. Chemical Society, Amer. Museum Nat. History, New York, N. Y.; Central Library, Syracuse, N. Y.; Historical and

Library Association, Yonkers, N.Y.; Amer. Chemical Society, Amer. Academy of Medicine, Easton, Pa.; Penna. Society to Prevent Cruelty to Animals, Girard College, Historical Society of Pennsylvania, Mercantile Library, Penna. Forestry Association, Messrs. Guy Hinsdale, William A. Ingham, Edmund J. James, J. G. Rosengarten, Julius F. Sachse, Philadelphia; Enoch Pratt Free Library, Peabody Institute, Baltimore, Md.; Department of Labor, Washington, D. C.; University of Virginia, Prof. J. W. Mallet, Charlottesville, Va.; Artillery School, Fort Monroe, Va., Elisha Mitchell Scientific Society, Chapel Hill; Mr. Charles Gildehaus, St. Louis, Mo.; Michigan Mining School, Houghton; Agricultural Experiment Stations, Kingston, R. I.; Albany, N. Y.; Geneva, N. Y.; Jacksonville, Fla.; Agricultural College, Miss., Knoxville, Tenn.; Denison University, Granville, O.; Agricultural Experiment Station, Lexington, Ky.; Iowa State Historical Society, Iowa City; Kansas University, Lawrence; Agricultural College, etc., Cheyenne, Wyo.; Colorado Scientific Society, Denver; Agricultural Experiment Stations, Fort Collins, Colo.; Tucson, Ariz.; Asociacion de Ingenieros, etc., Observatorio Meteorológico Central, Instituto Geológico, Mexico, Mex.; Observatorio Astronomico, Quito, Mex.; Sociedad Cientifica Argentina, Buenos Ayres, Argentine Republic, S. A.; Museo de La Plata, Argentine Republic, S. A.

A circular letter from the Naturhistorisch. Vereine der Preussischen Rheinlande, Westfalens und des Regierungsbezirks Osnabrück, Prussia, announced the death of its Secretary, Prof. Dr. Philipp Bertkau.

The Council reported that at the meeting held February 14 the Committee on Premiums had been named, to consist of Messrs. Pepper, Frazer, Ingham, DuBois, Morris, Wistar and Tatham.

Propositions for membership 1332 and 1334 were recommended to be postponed for further information, and 1335 and 1344 were recommended for approval.

It was recommended that the first meetings in February,

May and November be designated for the presentation and free discussion of subjects of broad philosophic interest, and that a Committee of five be appointed to make the necessary preparations therefor.

The recommendations of the Board of Officers and Council were approved and the Chairman referred the appointment of the Committee to the President of the Society.

The following deaths were announced :

Hon. Henry Reed, Philadelphia, February 23, 1896, æt. 49.

Dr. Owen Jones Wister, Philadelphia, February 24, 1896, æt. 70.

The President, by letter, announced that he had appointed J. G. Rosengarten to prepare the obituary of Rev. W. H. Furness, D.D., and that the appointment had been accepted.

The stated business of the meeting being the election of members, the nominations were spoken to, and the ballots cast, Secretaries Frazer and DuBois acting as Tellers.

The following papers were presented for publication in the *Transactions* of the Society :

An essay on "The Development of the Mouth Parts of Certain Insects," by J. B. Smith, Sc.D.

"A New Method of Determining the Perturbations of the Minor Planets," by Wm. McKnight Ritter, M.A."

On motion, referred to Committees for examination and report.

The Tellers being prepared to report, announced that

2278. Dr. A. E. Kennelly, Philadelphia ;

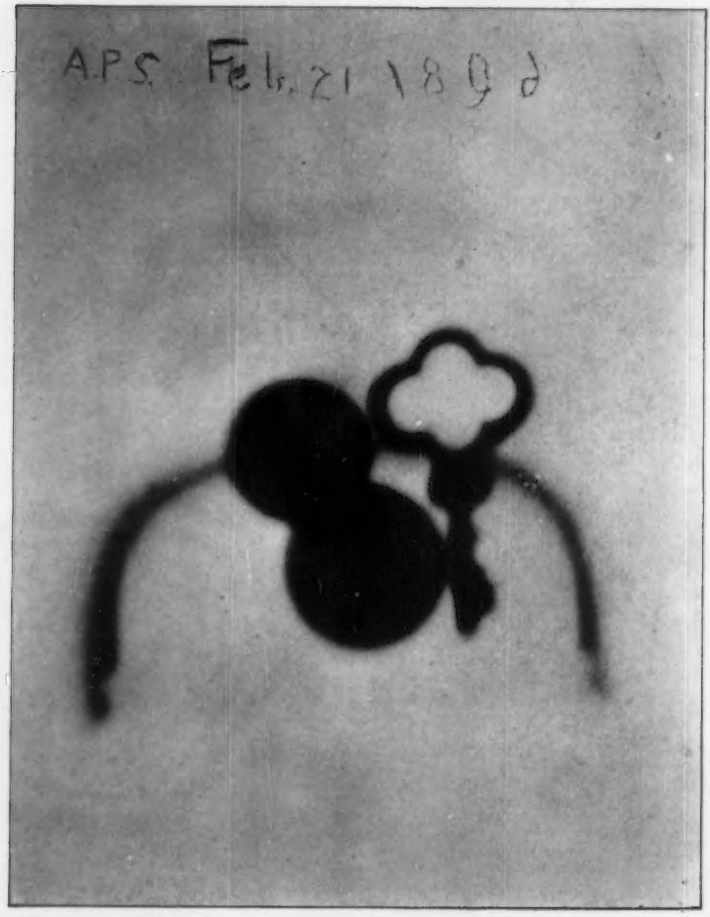
2279. Wm. Pitts Mason, Troy, N. Y.;

2280. Rev. H. C. McCook, Philadelphia ;

2281. Henry Pettit, Overbrook, Pa.;

had been elected.

The Society, on motion, adjourned.



PICTURE TAKEN DURING THE DEMONSTRATION OF THE RÖNTGEN RAYS AT THE MEETING OF THE AMERICAN PHILOSOPHICAL SOCIETY HELD FEBRUARY 7, 1896.

Remarks made at the Demonstration of the Röntgen Ray, at Stated Meeting, February 21, 1896.

Prof. Goodspeed describes his apparatus as follows:

In order to economize time it may be worth while for me to call your attention very briefly to the apparatus that we will use to-night, before beginning the reading of the regular paper. We have here two terminal wires which are supplied with the electric current from several storage batteries which are behind a screen. The electro-motive force is about sixteen volts. This induction coil which is to furnish the current to stimulate the tube has a primary resistance of about three-tenths ohm. The resistance of the secondary coil is about 3200 ohms, dead resistance. By passing the primary current through the small resistance coil and interrupting it frequently, as you all know, we produce an induced current in the high resistance secondary coil. It is the discharge of this induced current through the Crookes tube which you see here that produces the green phosphorescence and secondarily, probably, produces or sets up the form of energy with which we are to deal this evening.

In order to make a test case I will place this little pocketbook, with a couple of coins and an iron key inside it, upon a sensitive photographic plate, which is placed upon the table wrapped in several thicknesses of light-tight paper. The plate, as you will see, is three or four inches below the lower end of the tube. The tube is much larger than is usually seen; and for that reason, probably, is more efficient. The internal pressure is probably about one one-millionth of an atmosphere. The exposure may continue during the reading of the paper. Subsequently we will have the plate developed.

THE RÖNTGEN PHENOMENA.

Gentlemen:—Never before in the history of science has a new discovery commanded such intense and universal interest as that, some of the features of which we have met here to-night to witness. Less than two months ago, the civilized world was startled at the unofficial announcement that Prof. Röntgen, of Würzburg, had discovered a form of energy probably related to radiation, which would pass through many substances that were opaque to known forms of ether energy. An interesting point in this connection was that glass, ordinarily so transparent to light, seemed to be quite opaque to the new energy. Since the original paper of Röntgen has appeared, we have learned that the discovery referred to resulted from a series of experiments on fluorescence. The important pieces of apparatus that were used, and which we have before us this evening, consist of an inductorium with its secondary coil connected to a well-exhausted Crookes tube. A high degree of exhaustion is noted by the absence of a bluish halo about one

or both of the terminals. The internal pressure is about one one-millionth of an atmosphere. The earliest form of vacuum tube, constructed nearly fifty years ago, was exhausted to about one one-hundredth of an atmosphere, and on the passage of an electric discharge, glowed throughout its length with a purplish-blue color. As the efficiency of the pump increased, higher vacuum became easy and the phenomenon of the dark space about the cathode was described and exhibited to the British Association by Crookes in 1879. As the exhaustion is increased the dark space may enlarge so as to extend throughout the length of the tube. Under these conditions, the position of the anode is of little consequence, and under the action of the discharge the whole bulb becomes fluorescent with green or blue according to the kind of glass.

"Cathode rays" is a term applied to the disturbance which seems to start at the cathode within the tube, and extend in straight lines to the opposite side. These rays are capable of being deflected by a magnet, and were supposed by Crookes to consist of the molecules of the residual gas projected with great speed from the cathode terminal and impinging upon the walls of the tube. In the language of molecular kinetics, it may be said, then, that the mean free path of the molecule in one of these highly exhausted tubes, has become greater than the length of the tube. It was discovered in 1890 by Hertz that these cathode rays can pass through some solid substances, *e. g.*, aluminum, while others he found to be opaque. Lenard, the assistant of Hertz, in 1894, passed the cathode rays outside the tube, through a small aluminum window, placed in the wall of the tube opposite the cathode. This window had to be very thin to facilitate the issue of the rays, and yet thick enough, compared with its size, to withstand the pressure of the atmosphere. Consequently, the area was very small. Lenard also obtained shadow records on photographic plates by interposing, between the aluminum window and the plate, opaque bodies.

The cathode rays when impinging upon the Lenard window do not issue in a direction collinear with their former direction; but seem to spread in all directions like a beam of light passing beyond a very small aperture. The transparency of substances for these rays seemed to be closely related to their density. For example, in the case of gases, hydrogen was found to behave like oxygen if it were compressed until its density became equal to that of the oxygen. Transparency to these rays seemed to have no relation to electric conductivity.

With reference to leaving out the aluminum window and replacing it by merely the glass of the tube, Lenard said (*Electrician*, Vol. xxxii, p. 576): "On replacing the aluminum window by one of glass, it was found "possible to repeat all the essential experiments with equal success. But the aluminum remains the more suitable, not that it is the more transparent, but because aluminum is opaque to light, and more easily manipulated than glass of equal thickness." So we see that Lenard actually obtained results in about the same way that we are ex-

perimenting now. Dr. Oliver Lodge, of Liverpool, tried two years ago to repeat this very experiment, with a tube of rather thick glass, "Failing," to use his own words, "simply by reason of insufficient pertinacity." This is doubtless the case, since Lodge has lately repeated Röntgen's experiment with that same tube, obtaining results "through a quarter inch of wood and a sheet of aluminum, provided something like a half an hour's exposure is allowed" (*Electrician*, Vol. xxxvi, p. 438).

Opinions differ as to whether the rays used by Lenard were the same as those producing the Röntgen phenomena. As has been said, cathode rays are deflected by a magnet, while the Röntgen rays seem not to be. The Lenard rays, also, were shown to be capable of deflection by a magnet under certain conditions. Röntgen, himself, is of the opinion that the new energy is some form of ether wave motion perhaps longitudinal, and Lord Kelvin, I think, maintains the same views. Other English authorities seem to be divided between the ultra-violet theory and the longitudinal wave theory.

Dr. Lodge in a lecture before the Liverpool Physical Society, January 27, 1896, expressed himself as rather favoring the opinion that the Röntgen rays are highly electrified material particles, traveling with very great velocity. In a recent article (*Electrician*, Vol. xxxvi, p. 430), Lodge says, that "He permits himself to doubt and inclines to a sort of electrolytic impulsive propagation, through and by means of ordinary matter; in spite of the immensely important fact that Prof. Röntgen can detect in his rays no magnetic deflectibility whatever." In concluding the article referred to, Lodge says, "Meanwhile, the possibility, even the probability, that in these rays we have a new kind of radiation, even though it be only ultra-violet, so high up as to be comparable to the size of molecules, lends to these experiments a prodigious interest in the eyes of physicists, far surpassing the obvious practical applications which have gained the ear of the general public."

Since writing the above, Lodge has himself repeated the magnetic experiment with very great care, finding no deflection (*Electrician*, Vol. xxxvi, p. 471), and expresses himself as follows: "Consequently, the hypothesis of a stream of electrified particles is definitely disproved, as no doubt had already been done in reality by Prof. Röntgen himself."

It seems that Lenard had arrived at the conclusion that he was dealing with two classes of rays, as regards their deflectibility by a magnet. The question may still arise then, May not the Röntgen rays be the undeflectible Lenard rays?

The ultra-violet theory is said to be favored by Professors Schuster and Fitzgerald. One difficulty is, that some electrical conductors are practically transparent to the new radiation. To waves of light of every kind they ought to be opaque according to Maxwell's theory. However, the fact that gold and some other metals, when excessively

thin, are translucent has long presented a difficulty, which is only partially overcome by the assumption, that "the structure is not infinitely fine-grained, with respect to the size of the light waves." It may not be too much to suppose that these new waves are comparable in size with the molecules, or even the atoms, of matter.

The theory of Prof. Röntgen, already referred to, that the new energy is longitudinal ether-wave motion, surely must not be ignored, especially as it seems to be supported, among others, by the distinguished mathematical physicist, Prof. Boltzmann, of Vienna. There are difficulties in supposing the ether to be compressible, yet it must assume the effects of compressibility, if it is to transmit a periodic disturbance with finite speed.

Röntgen's own theory seems well supported by G. Jaumann (Wiedemann's *Annalen*, January, 1896), who has shown in a recent article that by a little change in Maxwell's equations, to satisfy the conditions of high rarefaction, which is met with in a Crookes tube, longitudinal ether waves are possible, which would possess many of the properties of the new rays.

That the new energy does not consist of cathode rays alone, seems to be proved by the remarkable experiment of J. J. Thomson, who placed a protected plate inside the vacuum tube, exposed to the direct cathode stream, and got no result (Lodge, *Electrician*, Vol. xxxvi, p. 473). The same experimenter has suggested an efficient and quick way of detecting the presence of Röntgen rays. An insulated metal plate electrically charged, either positively or negatively, quickly loses its charge when in the presence of the rays. This occurs even when the plate is entirely embedded in the best insulators. It follows, then, that all substances become electrical conductors, when under the influence of the Röntgen discharge.

Should the longitudinal ether-wave theory be demonstrated to be the true one, Prof. Röntgen's discovery would be the greatest of the age, and will open up a vast new field for experimental research, and will likely lead to more definite views concerning the nature of the luminiferous ether.

Soon after the announcement of this wonderful discovery, we began to experiment in the Physical Laboratory of the University of Pennsylvania, at first rather skeptically and quite in the dark as to the exact method of procedure. As the earlier statements implied the necessity of two induction coils, the primary of one connected to the secondary of the other, we were somewhat embarrassed as we did not have two that could be efficiently joined in that way. To show the importance attached to this point by early imitators of Röntgen abroad, let me quote a statement by A. A. C. Swinton, who, I am told, was the first in England to repeat some of Röntgen's experiments. He says (*Nature*, Vol. liii, p. 277), "So far as our own experiments go, it appears that, at any rate, without very long exposures, a sufficiently active excitation of the

Crookes tube is not obtained by direct connection to an ordinary Rhumkorff induction coil, even of a large size. So called 'high frequency currents,' however, appear to give good results, and our own experiments have been made with a tube excited by current, obtained from the secondary circuit of a Tesla oil coil through the primary of which were continually discharged twelve half gallon Leyden jars, charged with an alternating current of about twenty thousand volts pressure, produced by a transformer with a spark gap across its high-pressure terminals."

Having no such apparatus as this at the University, and thinking that possibly some indication might be obtained from a simpler arrangement, we left out the second coil and joined the tube directly to the secondary of the first coil. The coil we are using was constructed by Apps, of London. It has a primary resistance of about 0.3 of an ohm, and a secondary resistance of about 3200 ohms. The Crookes tube which is one of the collection in the physical cabinet at the University, is a shadow tube nearly twenty-five centimeters long and eleven centimeters in diameter at its larger end.

The first result that was unmistakably a success was obtained on Wednesday, February 5. A small slip of glass and a piece of sheet lead, together with a wedge of wood, were held in place upon a sensitive photographic plate by elastic bands, and the whole enclosed light tight in an ordinary plate holder. This was placed horizontally upon a table, eight or ten centimeters below the large end of the Crookes tube. An exposure of twenty minutes produced, upon development, a sharp impression of the objects, the glass and lead appearing opaque, while the portion of the plate covered by the wood was hardly less affected than the parts entirely unprotected. The sight was startling at first as every experimenter who gets the result for the first time can testify. This experiment was immediately followed by an attempt to obtain a skeleton view of the hand, the result of which will be shown by the first slide.

From that time until the present, many experiments of a varied nature have been tried, the object being to investigate substances with reference to their transparency; to detect, if possible, refraction or reflection; to determine the action of various crystals cut in different ways with reference to the optic axis; and a few experiments to test the possible efficiency of a special method of treating the sensitive film.

Early associated with the writer was Dr. H. W. Cattell, who obtained some very curious cases of malformation of the hand and fingers, and produced results which have proved extremely interesting from a surgical point of view.

Our experiments on crystals have not resulted in much that is interesting, except, perhaps, in one case which I will refer to presently. One plate exposed had upon it a tourmaline, a double image prism, a Nicol prism, an amethyst, an irregular quartz crystal, some mica discs,

and some quartz plates with parallel sides. These all seemed to be rather opaque, though I think the exposure was probably too short. We shall experiment in this line at another time.

The second slide shows the skeleton of a lady's hand, which, as far as I know, is the first that has been produced.

The third slide illustrates the difference in the density of the negative caused by times of exposure on the four quarters of the plate of five, ten, twenty, forty-five minutes respectively. During the exposure of each quarter, the rest of the plate was protected by metallic screens. The test objects on the plate, are: a circular piece of cork; a gold coin; a strip of magnesium tape; a piece of glass, and a piece of aluminum. The distance of the tube from the plate was about ten and a half centimeters during all four exposures.

The fourth slide shows the skeleton of a mouse, taken laid flat upon its back; the legs being stretched out and brought as near the plate as possible.

Slide No. 5 shows the density of the negative produced by five-minute exposures, at distances of two and a half centimeters, five centimeters, seven and a half centimeters, ten centimeters and twelve and a half centimeters respectively. The plate was protected by a screen of copper having a circular aperture about one centimeter in diameter.

Slides No. 6 and 7 show the density produced at a distance of two and a half centimeters, with exposures of one to five minutes. These slides were also prepared to demonstrate the efficiency of a plate especially sensitized by Mr. John Carbutt of this city for this work. He conceived the idea that the photographic plate might be rendered more sensitive to this energy, if the film were treated with some fluorescent substance. Mr. Carbutt very kindly placed in our hands some of the special plates, and your attention is directed to a comparison between a very rapid ordinary plate (Seed's No. 27), and the one especially prepared. The treatment throughout was precisely the same. The prepared plate seems to have been considerably more sensitive than the other.

Slides Nos. 8 and 9 show the results of tests to demonstrate the possibility of reflection or refraction, by means of two large diamonds set in a ring. First the diamond ring was enclosed in a flat purse with some coins, and certainly the result is very interesting, though, perhaps, it would be premature to say that anything new is proved by it. The ring was next placed open directly upon the covered plate, and exposed in two positions.

Slide No. 10 shows a possible application of the Röntgen process. Wishing to test the value of the method for detecting flaws in metals, the writer requested one of his associates, Dr. Richards, to have prepared three aluminum plates, four or five millimeters thick, with hidden holes, plugs, or any flaws that might seem desirable. Dr. Richards was asked further to prepare a detailed description of the plates, to sign and seal it, and to bring it with him this evening. The aluminum plates

have been examined by means of the Röntgen process, and it may be interesting if one of your members will open the envelope and compare the description therein, with the one that will now be detailed. The picture tells its own story pretty well, even to the uninitiated. No. 1 seems to have three circular holes, plugged up with some substance, doubtless aluminum, having the same radiographic density as the material of the plate. No. 2 appears to be perfect. No. 3 has two holes similar to those of No. 1, and a third stopped up through a portion of its length by some substance less transparent than the aluminum, perhaps a piece of copper or iron wire (Dr. Richards' Description of Aluminum Plates).

Our experiments during the last two weeks have been made at all times of the day and evening, sometimes in full daylight, and often with no light at all, except that emitted by the tube. The presence or absence of luminous radiation seems not to make the least difference in the results. We early learned that sharper outlines could be obtained by omitting the usual plate holder, and wrapping the plate in several thicknesses of orange paper. By this means actinic light was excluded, and the objects were brought nearer to the sensitive film. During this series of experiments, the writer has received much assistance and many valuable suggestions from his associates in the department, Dr. H. C. Richards, Dr. R. R. Tatnall and Mr. G. C. McKee.

In connection with this subject, it is desired to direct the attention of the gentlemen present to a remarkable coincidence which can hardly fail to excite interest. In the fall of 1889, the writer received a letter from a prominent gentleman in Philadelphia, asking him to call at a convenient and early date, to be presented to a friend who was desirous of obtaining facilities for some experiments in electric spark photography. On the occasion referred to, the writer had the pleasure of meeting Mr. W. N. Jennings, of Philadelphia, who for many years has been much interested in the photography of lightning. It was Mr. Jennings' wish to photograph electric sparks from various forms of apparatus, in order to compare the results with the lightning pictures which he had already obtained. It is needless to say that the series of experiments, begun at that time, have been continued to the present, as occasion and opportunity have made it convenient.

The particular meeting of interest occurred on the evening of February 22, 1890. Slides 11 to 14 show the result of some of our experiments on that evening. We photographed the brush from a large induction machine, by holding the uncovered plate in various positions near the poles. We also placed coins and brass weights on the plates, sparking them by means of the Apps induction coil in various ways. After finishing the experiments of this sort, the writer brought out from the cabinet quite a variety of Crookes tubes, and showed them to Mr. Jennings simply for his pleasure and amusement. The desirability of getting Mr. Ives to reproduce some of the color effects by means of his

beautiful method was suggested. A few days later, Mr. Jennings announced the results of the evening's work and mentioned that several of the plates that had not been exposed directly, but which were developed along with the others, were found to be fogged. He also mentioned one, upon which had appeared a mysterious disc, that he was quite unable to account for as the character of the impression was entirely different from those that had been obtained in the regular way.

The matter was forgotten until about ten days ago, when the writer asked Mr. Jennings to look over the records of our early experiments, to see if we ever exposed a plate entirely covered in the plate-holder. He immediately did so, and found the plate upon which had appeared the mysterious disc. A very reasonable explanation now is suggested. The disc is doubtless the shadow picture of one of the coins made while we were viewing the Crookes tubes. To add still more weight to this theory, we repeated, a few days ago, the experiment in the same way that it must have been made, if at all, on that interesting evening. The original plate and the result of the recent experiment, we have the honor of showing you here. Now, gentlemen, we wish it clearly understood that we claim no credit whatever for what seems to have been a most interesting accident, yet the evidence seems quite convincing that the *first* Röntgen shadow picture was really produced almost exactly six years ago to-night, in the physical lecture room of the University of Pennsylvania.

ARTHUR W. GOODSPEED.

UNIVERSITY OF PENNSYLVANIA, February 21, 1896.

Prof. Edwin J. Houston's Remarks were as follows:

It is unquestionably the fact that although natural truths cry aloud to the scientific inquirer, yet they may long remain unrecognized. We have heard to-night, in the excellent paper Prof. Goodspeed has read, that although the apparatus we have just seen was in the possession of the University of Pennsylvania, and although it undoubtedly long ago produced the Röntgen effects, yet they were undetected. I had myself a similar apparatus in the philosophical cabinet of the Central High School; and the Röntgen rays were unquestionably produced by it, but they were not recognized. Many a case of a curious shadow photograph, appearing mysteriously upon a plate believed to be good, strange shadows coming out, the cause of which could not be detected, were most probably some of these Röntgen photographs.

The paper we have heard has reviewed in so able a manner the comparatively few facts that are known concerning this peculiar form of radiant energy, that I may, in my remarks, be forced to repeat some of its statements, but it may, nevertheless, be of interest to you if I do so in other language.

The term cathode rays is applied to the stream of electrified molecules

that pass in a rectilinear direction from the negative electrode, or cathode, of a suitably exhausted vacuum tube. This peculiar effect is not observed to any marked degree until the residual atmosphere in the tube has a tension or pressure of but about the one-millionth of an atmosphere, or until that peculiar condition of matter in the tube is obtained, for which Crookes proposed the name of the ultra-gaseous, or radiant state. It appears that wherever the cathode rays strike the walls of the tube, or any suitable substance contained therein, they excite fluorescence. The cathode rays are deflected by magnetic flux. Indeed, they must be so deflected if they consist of streams of electrified molecules; for, their deflection by magnetic flux is a phenomenon allied to the deflection of a voltaic arc by a magnet, or the deflection of the active wires on an electromagnetic motor, by the flux from the field magnets.

Reviewing briefly the history of the Röntgen discovery, we will find some of the facts to be as follows; viz., Hertz showed that thin metallic films are transparent to the cathode rays. Lenard, an assistant of Hertz, who afterwards took up the investigation both in connection with Hertz and individually, placed an aluminum window in the tube so that the cathode rays impinged on it. You probably noticed, in looking at the radiation from the tube shown by Prof. Goodspeed, that the rays did not light up the entire surface of the tube, but that a spot directly opposite the cathode was markedly excited by the phosphorescence. That is the spot where a peculiar kind of radiation, called the Lenard rays, or the Röntgen rays, was observed; the Lenard rays in one condition of the vacuum, and the Röntgen rays in another condition of the vacuum. Assuming, that the cause of the Lenard or Röntgen rays is the impact of a molecular stream of electrified particles, most probably molecules, we may inquire as to their origin. They are evidently either disengaged from the substance of the negative electrode or cathode, or they are simply the molecules of residual gas in the highly exhausted tube. Inasmuch as Pupin has shown that electrodeless Crookes tubes, that is, tubes not provided with interior electrodes, produce the same effect, it would seem fair to believe that botht he Lenard and the Röntgen effects may be due to molecular bombardment of the molecules of the residual atmosphere. In these electrodeless tubes, pieces of tinfoil are placed on the outside of the tube, and the terminals of the Ruhmkorff coil being attached to them, discharges are produced by electrostatic induction corresponding to the discharges of the secondary of the Ruhmkorff coil, and all the effects of either the Lenard or the Röntgen rays are produced.

Lenard states that his rays are faintly visible to the eye outside the tube. They are, however, rapidly absorbed by the air, so that at a short distance from the tube they cease to be visible. The Röntgen rays, on the contrary, are invisible to the eye. Both the Lenard and the Röntgen rays produce phosphorescence in phosphorescent materials

on which they impinge; they both traverse opaque films of metal; they both produce actinic effects on photographic plates. That the Röntgen rays are something different from the Lenard rays is proved, I think, by the fact that they are not by any means so absorbable by air.

It may be interesting to know how Röntgen's original effects were obtained. He took an ordinary Crookes tube, or at least a tube containing the proper vacuum, and completely covered it with blackened pasteboard so as to render it light tight to ordinary light. He took a paper screen which he painted with a substance capable of being excited by fluorescence, a solution of barium-platino-cyanide. He then found that wherever this screen was impinged on by the Röntgen rays, it fluoresced.

Röntgen found, that his rays, like the Lenard rays, possess the strange power of passing through many substances opaque to ordinary light. It is generally believed that the source of the Röntgen rays is the portion of the glass tube which receives the bombardment of the molecules shot off from the negative electrode. In other words, the Röntgen rays are caused by the cathode rays. That they are not the cathode rays themselves is evident from a brief review of some of their characteristics.

1. The Röntgen rays are invisible to the eye.

2. They excite fluorescence. (In this respect, however, they agree with the cathode rays and the Lenard rays.)

3. They produce actinic effects. In this respect they agree with the Lenard rays, but are entirely differentiated from the cathode rays. A photographic plate has been placed inside a Crookes tube and the cathode rays have been caused to impinge on it. They failed to produce any actinic effects. There are clearly then these differences; the Röntgen rays produce actinic effects; *i. e.*, they possess the power of decomposing a photographic salt placed on a sensitive plate, and are not deflected by a magnet. This latter point has been confirmed recently by some very careful experiments made by Dr. Oliver Lodge. The apparatus would have detected any deflection had it existed.

There is, however, a marked similarity between the Lenard and the Röntgen rays. The source of both is believed to be the cathode rays. They each produce fluorescence; each possess the power of passing through substances ordinarily opaque, the opacity increasing apparently with the density, though not in direct proportion with the density. The Röntgen rays, however, differ in the valuable property of not being so readily absorbed. The Lenard rays, though not deflected by a magnet, in free air, are deflected by a magnet when they are caused to enter a highly exhausted chamber—at least, so Lenard states. It is said that Prof. Wright, of Yale, a careful student and one whose opinion is to be regarded, does not think that the Röntgen rays differ from the cathode rays. He rather looks on the Röntgen rays as strained cathode rays.

That the Röntgen rays possess three characteristics of ordinary light; viz., rectilinear propagation, as shown by their ability to cast shadows; the power of producing fluorescence; and the power of effecting chemical decomposition in a sensitive photographic plate. They differ from light, however, in nearly all other respects. If they are ether waves they may be transverse waves, which we know of; or they may be the long-looked-for longitudinal waves. They are, however, apparently incapable of reflection, refraction or interference, all characteristic of transverse vibrations. If they are transverse vibrations they belong to some part of the spectrum that we have not hitherto studied. In the opinion of some physicists they belong to a region considerably below the red; in the opinion of others they are exceedingly short wave lengths, possibly approaching atomic or molecular dimensions.

I have used in connection with my colleague, Dr. Kennelly, in the study of the Röntgen effect, both the character of apparatus described by Prof. Goodspeed, as well as other apparatus. Dr. Kennelly and I, charge a battery of Leyden jars with the discharge of a large Ruhmkorff coil; we get a spark discharge and a spark gap, and then use that spark discharge, which is an oscillatory discharge, through the primary of a Tesla coil. We thus obtain in the secondary coil an exceedingly high discharge and use this to excite the Crookes tubes. The Tesla coil was immersed in rosin oil. It seems from the experiments we have made that these very rapid oscillations are not so apt to injure the tube and apparently produce better results. However, in sharp opposition to this, I hear a rumor, though it is only a rumor, that at the Johns Hopkins University they are working in the opposite direction; viz., with very few oscillations of the primary per second. I hope Prof. Rowland, who is conducting these experiments, will soon let us know what he is doing.

Mr. Edison has been a tireless investigator in this field of physical research.

Prof. Schuster is decided in his opinion that the Röntgen rays are not the cathode rays. He agrees that the point of origin is where the stream of negatively charged molecules strikes the glass. Prof. Whiting finds gum to be the most transparent and rock salt the most opaque substance to the action of the rays. Prof. J. J. Thompson states that the cathode rays are incapable of affecting sensitive photographic plates. We all know that the ultra-violet rays, which some think are the same as the Röntgen rays, will effect the discharge of a negatively excited body. Prof. J. J. Thompson has shown that the Röntgen rays will effect the discharge of either a negatively or a positively excited body, and this whether or not the body is surrounded by the highest insulating substances known to the electrician, like vulcanite or parafine. Of course, I know that most of you will know what this means; viz., that a leak takes place in those substances; or, in other words, that while the Röntgen rays are passing through these substances they become conductors of electricity.

Mr. Carbutt: Do I understand you to say that no positive results have been obtained yet at the bell of the receiver of the exhaust pumps?

A. I say that I understand that no sensitive plate has yet been obtained, which, placed in the Crookes tube, will have any actinic effect produced on it by the cathode rays. When they pass outside the tube they are no longer cathode rays.

Q. But if placed on the bell of the receiver of an exhaust pump?

A. I have not tried that.

Q. Just to-day I made the experiment of exposing a pair of steel scissors; and in five minutes obtained a strong negative effect, getting my rays from the negative pole.

Q. Then they went through the glass of your receiver?

A. No, sir; they struck right on the metal scissors.

Q. Where had you your photographic plate?

A. On a bell receiver. I used no Crookes tube, nothing but just the rays as they came down from the negative pole. The plate was lying on a little table as connected with the positive pole and the rays were seen traveling down on the plate on which were laid the scissors.

A. I think you had an effect very much like the electric discharge effects shown on the screen to-night. I believe that a great many statements made concerning the ability of other sources of light to produce Röntgen rays are due either to heat effects, or to electric effects.

Dr. J. Cheston Morris asked if Edison was experimenting with celluloid plates. Prof. Houston said he did not know.

Remarks by Mr. Julius F. Sachse were as follows:

So far as the photographic properties of the new X rays of Röntgen are concerned, it is yet a question whether they will ever be of any practical value or use for photographic purposes, as the term is usually understood.

The fact that these rays can neither be refracted, condensed nor dispersed, is a fatal objection to their application to photography.

It will be noticed that all of the registered or permanent results obtained and shown here this evening are by no means photographs in the ordinary sense of the word; they are merely fixed shadows or "sciographs" obtained by the interposition of a sensitive gelatine plate.

I do not wish to be understood as depreciating this new factor in physics, nor to appear skeptical as to any practical results that may be forthcoming in the future. It is now certain that a great discovery has been made by Prof. Röntgen, notwithstanding the fact that these identical rays have been produced thousands of times, in nearly every physical laboratory in the world, and that it only needed the neighborhood of a luminous film to reveal them, and to do this was Prof. Röntgen's opportunity. The step to substitute a sensitive plate to register the shadow was a short one, and we have here to-night a practical demonstration of the results.

I now wish to call your attention to another peculiarity of the new Röntgen rays, that has just come to my notice, and had time permitted, I should have had the specimens here to illustrate my remarks.

The most exhaustive series of photographic experiments thus far made in connection with the Röntgen rays are the investigations at the Imperial experimental institution at Vienna (K. K. Lehr- und Versuchsanstalt für Photographie in Wien). Thus far no results have been obtained greater than the original skeleton hand of Prof. Röntgen. Scientifically, however, the curious fact has been learned that the actinic action of the so-called X rays is dependent to a great extent upon the medium or support that holds the haloid salts in suspension.

It appears that for some reason as yet unknown the new Röntgen rays have a peculiar affinity for a sensitive plate whose support consists of animal matter or gelatine. Now if we take a plate of equal sensitiveness, but substitute collodion for gelatine, and expose it to the action of the X rays, no effect whatever is produced. The rays seem to be absolutely inert the moment any medium is substituted for the animal support of the ordinary commercial dry-plate.

This series of experiments at Vienna consisted in testing the ordinary bromo-argentic gelatine dry plates of different degrees of sensitiveness together with argentic-iodide collodion (wet) plates—bromide collodion emulsion, and moist eosine bromo-collodion (Albert emulsion) and argentic chloro-bromide collodion plates, the latter developed with an alkaline solution.

The result of this series of experiments was that the Röntgen rays made little or no impression upon any variety of the collodion plates whether wet or dry, while upon the contrary every variety of gelatine plate, no matter whether sensitized with argentic bromide, iodide or chloride, proved a ready recorder for the Röntgen rays. The most effective plates were what are known in Germany as the "Schleusner Rapid" bromo-gelatine dry plate; they are equal in rapidity to our American plates "Sensomiter 23."

It appears from this series of experiments that the most marked difference was found in the comparison of a chloro-bromo-gelatine dry plate with a collodion wet plate, both of which were carefully tested as to their equal sensitiveness by daylight prior to being exposed to the effect of the X rays. Where the dry plate with alkaline development proved a success, the wet plate with an acid-iron development was an absolute failure.

Another peculiarity shown was that an alkaline development in every case gave better results than a neutral or acid one. Then again when a dry plate of the kind giving the best results was moistened or dampened before exposure, the sensitiveness for the X rays was greatly diminished.

Here perhaps we may find a solution to the problem why it is that none of the American results obtained by use of the X rays thus far have been equal, either in distinctness of outline or reproduction of detail, to the German sciographs. It may be to the humidity of our atmosphere, more

than to the quality and character of our photographic dry plates, or the lack of skill of our experimenters, that we have to look for either cause or failure.

It will thus be seen that many new factors enter into the photographic development of the new forces. Conditions seem to arise at every turn that are entirely foreign to those encountered when we work with either solar or artificial light, and this independent from the optical features which I have mentioned.

Now the question naturally presents itself as to which kind of sensitive plate, or medium, should be used to obtain the maximum results of the actinic action of the X rays, or in other words, by what means can we obtain the best permanent Photo-Sciographs?

As to the difference between the action of the X rays upon gelatine and collodion I would venture the theory that if these results are confirmed by experiments here, that it is due to the fact that while gelatine arrests the X rays, they pass through or penetrate the collodion film. It this should prove to be the case, it would indicate the use of double-coated plates, or of a stripping film upon a support impervious to the X rays, such as a sheet of lead. By such means perhaps photographic results of still greater value might be obtained. I will here state incidentally that the Schleusner plate used in the German experiments is coated somewhat heavier than the average American plate.

I now come to another aspect of the possible development of the photographic properties of the new forces; an experiment thus far untried in connection with the Röntgen rays. For this purpose I will turn backward and take recourse to the original principles of heliography, and suggest a series of experiments wherein we substitute for the gelatine dry plate a highly polished sheet of metal, subjecting it to the action of the X rays in the usual manner, and then seeking to develop the impinged image, if there be one, with the fumes or vapor of mercury or iodine, or the two in combination, a process well known to photo experts of the old school.

Tests should also be made upon the silvered copper plate coated with the vapor of iodine and bromine and developed with the fumes of mercury (the old daguerreotype process); or upon plain sheets of polished copper, silver or tin, and developed either with vapor, or by the application of heat to the reverse side of the plate; a process known as "Hunt's Thermography."

The above experiments are well worthy of a trial in connection with the development of what may be called "photo-sciography."

In conclusion I will call your attention to a curious coincidence. It was in this room just fifty-three years ago during the centennial celebration of this Society (May 29, 1843) than an almost identical topic formed the theme for discussion, viz.: Moser's theory of "Invisible photographic rays," a theory which was then attracting great attention in scientific circles on both sides of the Atlantic. Remarks upon the subject were

made by a number of members present, among whom may be named Dr. Paul Beck Goddard, Joseph Saxton, Prof. Henry and Prof. James Rodgers, all names that are still held in high esteem in the scientific world.

While upon the subject of Moser's theory, I will state that there have of late come to my notice several cases which seem to confirm his theory of latent light, or invisible photographic rays. The most marked instance was where a number of platinum prints were packed away and laid undisturbed in the dark for several months, and in several cases had reproduced themselves or formed a reverse positive picture upon the surface of the white plate paper mount which laid immediately over the print I merely mention this matter at this time so as to place it upon record, as I expect to bring it before the Society in a more formal way in the near future. As a fitting close to this paper I will quote the language of Robert Hunt, used in connection with Moser's theory and read here half a century ago, as it will apply with equal force to the theory of the unknown waves known as the X rays of Röntgen: "As a subject of pure scientific interest this discovery promises to develop some of those secret influences which operate in the mysterious arrangements of the atomic constituents of matter, to show us the road into the hidden recesses of nature's works, and enable us to pierce the mists which at present envelope some of its most striking phenomena. It has placed us at the entrance of a great river flowing into a mighty sea, which mirrors in its glowing waters some of the most brilliant stars which beam through the atmosphere of truth."

Referring to the paper read by Mr. Julius F. Sachse, Mr. Joseph Wharton asked:

Q. Will the gentleman please explain more fully what is the action of the X rays upon the more sensitive gelatine film as contrasted with their action upon the collodion?

A. I have not had the time to verify it by experiment; but as the case stands at present I cannot explain it except that the rays pass through the collodion film: they fail to arrest. That is the only explanation I can give at the present moment.

Q. That seems to be somewhat at variance with many of the observations that we have had set before us to-night: namely, that a number of so-called colloid bodies seem to be pervious to the ray; while a number of the crystalloid bodies seem to be impervious. Here are pitch, gum, leather and several other bodies which are pervious to the ray (all colloids); while quartz, rock salt and other crystals (the speaker naming several) all appear opaque to the ray. It may be worth while to bear in mind, in future investigations, the question whether there may be a line drawn between colloids and crystalloids in transparency for the new ray, and if so to search for the reason of that distinction.

Remarks of Prof. Robb, of Trinity College, Hartford:

We are all indebted to Prof. Goodspeed for a very interesting paper and must congratulate him. There is certainly a great deal of interest in those slides. The first thing that attracted our attention in Hartford about our dry plates was the fact that on a great many of them we noticed second images which were clearly defined, but fainter than the first, having decidedly the appearance of the ordinary halo images of ordinary photography. At first glance one might think that was due to reflection. I am sure it was not due to any movement that occurred in the plate; and I am sure it was not due to a violet region of photograph. I think exposures in bright light are a very dangerous thing. It is very possible to get shadow photographs through any of the commercial plate colors; but in a great deal of our work where we have worked in the ordinary light we have taken the precaution of using an aluminum cover of over a thirty-second of an inch; and we get second images to the same extent using the aluminum cover. Of course there are various explanations of it. It might be from fluorescence or other things that may suggest themselves to you.

With reference to Prof. Moser's slow plates giving better effects than rapid plates, that has not been our experience. We gave up the most rapid plate. We experimented with the most rapid plate that we could get, and we found some twenty of the plates were apparently light-struck; and finally we settled the question they were not light-struck; they were electric-struck by the brush discharge at the lower end of our Crookes tube.

One thing is very apparent to all of us that have been doing much work in this line—that the induction coil needs improvement. For as at present constructed they are not made to run continuously for twelve hours. They are all right to run for a few moments for showing off Crookes tubes; but platinum terminals soon wear out or become hot; and we have to put on new ones. In that connection I have a very good idea, due to a mechanic who does a great deal of work for me, which I will show by a sketch. The platinum point is about a quarter of an inch long ordinarily and is attached to the end of a tube having a thread on it and gradually wears away. Instead of fastening that piece of platinum directly under the tube we take a piece of platinum wire four or five inches long and place it on the end of a second metal rod which screws into the first. In that way, instead of having simply a quarter of an inch of platinum to wear off, we have some four or five inches at our disposal; and in the next place the heat is dissipated long before it gets to the soldered joint.

I think in connection with these photographs, there are shadow photographs; but it is remarkable what an amount of detail we can see on some of it. I have a photograph of a razor taken inside of the case which is interesting to see. When we looked at it, it was very bright in the middle of the razor—more light coming through there than at the edge.

One of my students said it must be a hollow-ground razor; and so we found it upon measurement. The photograph that we saw on the screen by Prof. Goodspeed of the aluminum plates with various holes bored in them was interesting, both as showing what can be done in the case of aluminum and what may be done in the case of other metals. From anything we know now as to the Röntgen rays, it will be impossible to tell much about armor-plating or anything of the kind; or about the molecular construction of any considerably thick pieces of the more opaque metals; but it does seem as though we can discover forms of ether vibration that will go through aluminum and go through hard rubber, and other forms that will go through pitch and things of that kind, and that certainly some day we are going to discover some form of ether vibration to which iron may be transparent. Of course we can all see what a tremendous application that would have in the mechanic arts.

We have one or two rather interesting photographs from a medical standpoint, showing its possibilities. Two or three of the students in photographing their hands discovered differences. One case of sesamoidal bone is very apparent, between the thumb and fore-finger of one of the students' hands; and then just two or three days ago we had a laboring man who was out of work from an injured hand; had been injured in a runaway accident and had gone to a local physician who has quite a reputation for doing poor work; had his hand treated; and it was never getting well; and we put it under the Crookes tube and, sure enough, there was a partial dislocation and a fracture which had never been attended to properly. Of course he was very glad to have us point out how to remedy it.

We have experimented slightly with a very interesting Crookes tube. We made a Crookes tube out of an ordinary lemonade shaker—whisky shaker—I don't know what you call it down here—with a hard rubber end in it; and the results have been very negative. We have never gotten any shadow photographs with it. We have simply taken three or four photographs with it.

Mr. John Carbutt's remarks were as follows:

My interest in the new Röntgen rays has been from the first reading of them. Being so interested in photography, when reading of the wonderful results produced by Prof. Röntgen, I naturally saw that there was going to be a much larger outlet for dry-plates. Outside of its commercial value I naturally took an interest in its scientific aspect; and the first thing that struck me was the great length of time for which the objects had to be exposed to the Röntgen rays. I therefore made it my business to investigate and to see whether or not a plate could not be produced which should be more sensitive to the Röntgen rays; and, as mentioned by Prof. Goodspeed, I experimented with the fluorescent substances, having experimented with numerous dies in the making of anthochromatic

plates. I knew that several of them gave off a great deal of fluorescence. I have only produced plates printed on glass; but I shall take up a line of experiments at once by producing some on thin celluloid; because, for the physician and others that have cases to tend where the flat plate would be very difficult to use, the celluloid can be enclosed in an envelope —sufficiently opaque to ordinary light—and can be bound around the elbow or the shoulder or any part sufficiently round where a plate would have to lie flat; and I think it would find in that case several uses. I have been experimenting with some professors (which matter I am not at liberty just now to mention) when I made a sciograph negative of a woman's hand in twenty minutes, plates as large as 14 x 17 being used. A film of the same size could be bound around the back, for instance; and I think in that way that possibly the celluloid film (it is $\frac{10}{1000}$-ths thick) may possibly come in use. As it has been mentioned that Mr. Edison has been using slow to quick plates, I have not as yet experimented with anything slower than a very rapid plate and am inclined to increase its sensibility; and I think that in a measure I have succeeded, as Prof. Goodspeed has shown you. Since the sensibility of these rays is a subject that requires both study and experiment, I do not propose at the present moment to say that I fully understand all of its requirements; and it is in its experimental stage. I shall not let the matter drop; I find it very difficult to find any tubes that are giving the proper X rays. The one that Prof. Goodspeed is using, so far as I have seen, is the best one that I have come across. I have been using one to-day with which I gave a full half-hour's exposure and got no results. The reason was explained to me to-night in the remarks that were made that when a blue or a purple color comes from the negative or cathode end of the Crookes tube it is not efficient in giving off the X rays. There is no doubt that a great many professors who are trying these experiments and getting negative results are working with inefficient Crookes tubes.

Remarks of Dr. William Pepper were as follows:

I rise only to occupy the attention of the Society for a single moment. In pursuance of the suggestion of Dr. Minis Hays to me, we owe very much of the pleasure of this evening's discussion, he having suggested that I write to some friends in Canada; and as a result of it, I present from Prof. Cox, of the MacDonald Physics Building at McGill University, Montreal, this brief note, accompanied by these four very excellent photographs illustrating the application of this method to surgical diagnosis.

"The MacDonald Physics Building,
"McGill University,
"Montreal, February 18, 1896.

"*Dear Sir*:—Dr. Shepherd has sent to me your letter expressing a wish to have some of our photographs for the meeting of the American Philosophical Society on the 21st.

"Our results have been in no way peculiar except that we were fortunate in making a successful application to surgery almost at the start. I have nothing to describe in the way of new methods. In fact there seems at this moment to be nothing known or tried that was not suggested in Röntgen's original paper.

"I am forwarding as likely to be of most interest a proof of the negative showing the revolver bullet between the tibia and fibula of a man's leg. This was obtained on February 7, four days after my first photograph. The print shows a copper wire fastened around the leg above as a fiducial mark;" (here Dr. Pepper interpolated as follows to the closing of this parenthesis: "then on the Röntgen sciograph should be seen between the tibia and fibula both in the positive and negative the small darker shaded area indicating the position of the bullet") "and the flattened bullet between the bones. The latter was extracted next day; and the patient is now nearly well enough to leave the hospital. The bullet was two inches deep in the flesh and had been flattened into a ragged-edged disc with a groove where it was lying against the bone. It had been in the leg since Christmas night. Its position was guessed at; but the photograph converted a surmise into a certainty. On the same night, February 7, we obtained the hand of which I send a copy. It was interesting not only for its good definition (for a fourth attempt), but because it shows the rare sesamoid bones on the thumb and little finger. It belongs to a champion canoeist.

"The main ideas I have found time to try—increasing the sensitiveness of the plate by (1) placing a fluorescing screen inside the holder in contact with it: (2) soaking the plate in the fluorescing substances—I now see have been successfully carried out by Geissler, of Bonn; so that I have nothing new to interest your Society.

"Believe me,

"Very truly yours,

"JOHN COX."

"The idea was to excite sympathetic fluorescence and gain intensity by resonance."

Dr. Pepper, continuing with original remarks:—As to Mr. Carbutt's remark as to obtaining flexible discs for curved surfaces and this (from Prof. Cox) interesting contribution as regards the diagnosing of internal conditions, I would say the excitement has spread the world over: every day I am receiving numerous letters, telegrams, visits from people at a distance, coming to ask whether it has yet reached a point to become an aid to internal diagnosis. I will not at this late hour occupy the attention of the Society by calling their thoughts to the obvious, the very great difficulties of this method. The tissues which are inaccessible to the hand in palpation are guarded so often by bony surfaces that the danger of shadows existing—which will be almost more confusing than the difficulties which surround our present means of diagnosis

—is very obvious. The field of investigation is of enormous proportions. The assistance of Prof. Houston and his associate, Dr. Kennelly, is promised in entering on an elaborate series of investigations in this direction. Whatever may be the result, we promise ourselves the pleasure of submitting them at a later period to the attention of the Society.

I have also here a few photographs of Dr. Henry Cattell; but as most of them have been published before I do not know whether he would care to show them at present.

Mr. Wharton exhibited a tube containing argon produced by Lord Rayleigh, which was presented by him to Dr. Theodore Wm. Richards, of Harvard University. This tube being arranged for sparking was introduced into the current of a Ruhmkorff coil, where it made a fine display of color.

A number of the members examined this with a spectroscope provided by Dr. Goodspeed, and thus observed very clearly the characteristic lines of argon.

Stated Meeting, March 6, 1896.

President, Mr. FRALEY, in the Chair.

Present, 24 members.

Mr. Henry Pettit, a newly elected member, was presented and took his seat.

Correspondence was submitted as follows:

Letters accepting membership from Dr. A. E. Kennelly, Philadelphia; Prof. William Pitts Mason, Troy, N. Y.; Dr. Henry C. McCook, Philadelphia; Mr. Henry Pettit, Overbrook, Philadelphia.

Letters of acknowledgment from Prof. A. E. Nordenskiold, Ph.D., Stockholm, Sweden (143, 146); R. Accademia di Scienze, etc., Modena, Italy (143, 144, 145, 146); Buffalo Library, Buffalo, N. Y. (148); Dr. Albert P. Brubaker, Philadelphia (147, 148); Hon. J. D. Cox, Cincinnati, O. (148); Colorado Scientific Society, Denver (148); Bishop Crescencio Carrillo, Merida, Yucatan (148).

Accessions to the Library were reported from the Linnean Society of N. S. Wales, Sydney; Société Hollandaise des Sci-

ences, Haarlem, Holland; K. D. Geographische Selskab, Copenhagen; Société R. de Géographie d'Anvers, Belgique; Gesellschaft für Erdkunde, Berlin, Prussia; K. Gesellschaft der Wissenschaften, Göttingen, Prussia; Società R. di Napoli, Italia; Dr. E. T. Hamy, Paris, France; Philosophical Society, Cambridge, Eng.; Theological Seminary, Andover, Mass.; Academy of Natural Sciences, Indian Rights' Association, Prof. William F. Norris, Philadelphia; U. S. Naval Institute, Annapolis, Md.; Agricultural Experiment Stations, Atlanta, Ga.; Las Cruces, N. M.; Historical Society, Los Angeles, Cal.; Observatorio Central, Xalapa, Mexico; M. Alberto Sanchez, San Salvador, C. A.

A crayon portrait (framed) of Hon. Eli K. Price was presented to the Society by J. Sergeant Price, Esq.

The committees appointed to examine the papers, "A New Method of Determining the Perturbations of the Minor Planets," by Wm. McK. Ritter, M.A., and "On the Development of the Mouth Parts of Certain Insects," by John B. Smith, reported in favor of their acceptance, and on motion they were referred to the Publication Committee for action.

Dr. Frazer then read a paper by Dr. Edw. Pepper, entitled "Eucalyptus in Algeria and Tunisia from an Hygienic and Climatological Point of View."

The subject was further discussed by Prof. Houston, Dr. Brinton, Dr. Wm. Pepper, Dr. Frazer, Prof. Cope and Dr. Morris.

Dr. Morris, on behalf of the Curators, acknowledged the receipt of the shadow picture, and the photograph from it, taken by Prof. Goodspeed during his demonstration on Feb. 21, and by permission of the Society was allowed to present his views on the subject.

It seems to me that such pictures should be called, not skiagraphs, or photographs, but electrographs; as they may be produced under various circumstances involving absence of light, but always the presence of some form of electrical energy—such as frictional electricity from the driving belt of a wheel, or a magnet (as has been done in Baltimore), or the direct rays of the sun in the presence of substances opaque to light and heat.

It seems also to me that we have evidence, apparently convincing to our senses, of a current or flow of a stream of some sort through the

Crookes tube, *e. g.*, the rapid rotation of the radiometer when exposed to it. This current or stream, of whatever it may be composed, is striking with great intensity and velocity more than four hundred million times per second against a thin film of glass which is not in a normal condition of equal pressure on both sides—on one side is a vacuum more or less perfect, on the other the whole pressure of the atmosphere. Such rapid blows cannot do otherwise than place the glass in an electrically excited condition—precisely like that of the plate of an ordinary electrical machine. As the exciting cause in this case is a current of negatively electrified molecules of air, the inner surface would be negative, and the outer intensely positive, and this would induce corresponding conditions in all neighboring bodies. The current might be very small, but of very high potentiality; hence would penetrate deeply these surrounding bodies, but would also produce in them all the phenomena of induction. To this excited condition of the glass film of the Crookes tube we may refer the phenomena of phosphorescence, fluorescence and heating, which ensue by the transmutation of forces—just as when a stone is thrown into a pond waves of various size and frequency will be seen to be propagated and interfere with each other. That induction is the cause of the formation of the picture is rendered probable by the fact that the reduction of the silver salt takes place next to the glass of the photographic plate, and not on the free or gelatin surface; and I would suggest as worthy of experiment whether the same effect would not be produced through a series of similar plates and not only on the uppermost one. Prof. McFarlan, of Easton, has shown beautiful results proving the radiation of the energy from the cathode of the tube, which also accord with the induction hypothesis. With regard to the useful applications of these rays, they seem to me to afford a rational explanation of some of the benefits of the currents of induced electricity on nutrition and other vital functions, which those of us who have employed it in our medical practice have often observed without being able fully to explain, and which we can therefore use more intelligently and beneficially hereafter. So also with the effects of the direct sun-rays, or sun-bath, known from ancient times.

The plate shows the edge of the coins and other metallic bodies not clearly defined, but surrounded as if with a shadow, or shading off; this, when examined closely, seems to be composed of fine lines radiating from the coin or metal.

I therefore believe that the phenomena in question will be found to be due to an induction of statical electricity, in great measure if not entirely.

It may be well also to call attention to the fact that while sound, heat and light can be reflected, refracted, transmitted or absorbed, no similar phenomena have as yet been shown as to electric, galvanic or magnetic forces.

New nomination for membership 1346 was read.

The President announced that he had appointed Dr. Pepper, Dr. Frazer, Mr. Ingham, Mr. Jos. C. Fraley and Dr. Hays the Committee for the special meetings agreed upon at the last meeting of the Society.

The Society was adjourned by the President.

Eucalyptus in Algeria and Tunisia, from an hygienic and climatological point of view.

By Dr. Edward Pepper.

(Read before the American Philosophical Society, March 6, 1896.)

INDEX.

Chap. I. Division of Algeria and Tunisia into three zones, as regards climate, water, trees, health and population.
Chap. II. Chronological facts relating to the growing of eucalypti in Algeria and Tunisia.
Chap. III. General and special advantages of these trees. Limitations of their uses, and objections to them.
Chap. IV. Species and varieties most serviceable in Tunisia and Algeria.
Chap. V. When, where and how to grow them.
Chap. VI. Commercial value of eucalypti.

I.

Division of Algeria and Tunisia into Three Zones as Regards Climate, Water, Trees, Health and Population.

Division of Algeria and Tunisia into three zones as above indicated.

Algeria and Tunisia are properly divided into three zones as regards climatological, hydrological and botanical, as well as hygienic and ethnographic conditions.*

The southern zone, The Sahara, consisting generally of a vast area of sand, moving and yet in parts solidified as by petrifaction (hamada), inhabited by semi-barbarous and roving tribes; and of oases of date-palms, inhabited by settled and less barbarous communities.

The middle zone comprises the high plateaux, or steppes, covered with a wild vegetation (herbaceous, fructiferous and rarely arborescent)

*As regards purely hydrographical conditions, these countries are divided into only two zones: the basin of the Mediterranean and that of the desert, all water not flowing in the one flowing in the other direction. But as regards practical hydrology or hydroscopy and its influence on the climate, these colonies are, as stated, properly divided into three zones here described.

and sustaining numerous flocks of sheep and camels; also sparsely inhabited.

Finally the northern zone, or coast region, El Tell, is generally cultivated and much better watered and wooded, and has both plains and valleys, hills and mountains. Here the European population of three or four hundred thousand only slowly increases by birth as well as by immigration, among three or four million more prolific lowland Arabs and Kabyle mountaineers.

The year has but two seasons.

The Algerian and Tunisian year has but two seasons: the dry and the wet. The former or summer comprising three rainless months, July–October, and the latter or winter months, October–March, offering generally short and frequently heavy showers and rains, and four months of showers lighter and fewer as the season draws to its close. The transition between these seasons is often sudden, an almost vertical sun radiating great heat over the land as soon as the cloud-screens have disappeared from the atmosphere.

Local climates abound in the coast region.

A peculiarity of the coast region (the Tell) is the diversity of the local climates (in most cases improperly called artificial) due to geological and geographical conditions, such as the nature, conformation and lay of the land as regards the higher hills and mountains, valleys and rivers, the sea, lakes, etc.; and also to orographical characteristics, such as the height of the mountains, the depth of these valleys, etc.; as well as to hydrological facts, such as the presence, absence, abundance or scarcity of the waters, flowing or stagnant, either above or underground. These local climates also depend on the extent of the surface cultivated, and, to a lesser degree, on the nature of the plants grown. A soil left to, or returning to nature, such as that of the most northern Africa after the Arab conquest, is ever harmful; the Corsican mâquis, the Indian jungle, the African brush, the Australian bush, etc., are among the strongholds, and so to say the lairs of disease, especially of malarial disease.

Moreover, the winds that blow over a country exercise the greatest of all influences on climate and vegetation, and consequently on health. There are parts of northern Africa, as of Asia, of America and of Australia, and even limited parts of Europe, where a progressive population can never dwell, while the physical causes actually at work exist.

In the coast region of Algeria the same communes, nay even the same towns frequently exhibit different climates in their different parts. Thus, Algiers itself has distinctly two local climates: that of the Bab-eloüed and Marengo quarter is more bracing, that of the Bab-Ozoun and Isly more relaxing. These differences are of great importance to the sojourners generally and especially to invalids passing the winter in Algiers; and they are even more marked in the suburbs of St.

Eugêne with its northeastern and of Mustapha with its exposure southeastern, the latter being under the predominating influence of the extensive Bay of Algiers.

The mountains of this region have generally a dry and bracing air, with severe cold in winter in the higher altitudes, where snows last through many months, and where even cases of frozen extremities are not rare. These highlands would in summer have great attractions and advantages as climate stations if they possessed suitable accommodation for sufferers from the heat, debility, or malaria, prevalent in so many parts of the lowlands. Such sanataria would, in many cases, do away with the necessity of the yearly trip to Europe, habitual with an ever-increasing class of the population.

General climate the sum of the local climates.

The general climate in this region, as elsewhere, is but the sum of local climates, with their differences of heat, cold and dampness. As to the latter, it is noticeable that the atmosphere is almost saturated on and near the seashore during the summer, excepting when the wind blows from the desert lying to the south and principally to the southeast; the dampness being at its maximum when the northeast wind blows; while further inland the dampness diminishes and finally disappears. Thus, on the seashore and in its proximity the air contains less moisture in winter, although it is the rainy season then and the moisture is most manifest; that of summer being more perfectly dissolved in the air, and (excepting when the northeast winds blow) being recognized more readily by the hygrometer. The rainy winter months are naturally the damp months in the interior.

Algeria and Tunisia were well-wooded and healthier in ancient times.

In the prosperous days of old, Algeria and Tunisia were relatively thickly wooded, as were most of the countries bathed by the Mediterranean, and they were doubtless more healthy than now. The mountains and hill-sides, the plains and alluvial levels of the Tell, as well as some parts of the high plateaux, have appropriate soil for trees, which in the former region would still abound if not systematically ruined by the fires kindled by the Arabs,* and the abuse of pasturage, their almost universal waste of wood, resins and barks, among which may be cited valuable cork and tannin barks.

Extent of woods remaining as compared to that of France.

Actually the fourteen million hectares of the Tell have less than fourteen hundred thousand hectares of forests left, offering scrub or brush, and less frequently, although there are fine exceptions, forest trees; as compared to the seven or eight million of

* And yet new growths frequently spring up from the ashes of these fires, under the teeth of the cattle, so to speak; but only to be fired again to produce new pasturage, until finally in this weakly and intermittent existence the beneficial influence exercised on the climate by trees is reduced to a minimum.

hectares of generally better woods remaining in France, with its surface of about fifty million hectares.

The above fact is quite sufficient to justify the alarm-cry of the Ligue du Reboisement, which, alas, has so far been "vox clamantis in deserto."

Consequences of the ruthless destruction of trees which is still going on.

This ruthless destruction of forests, groves and frequently of scattered trees is still going on and is the main cause of the diminution of the rainfall, the exhaustion of the soil and of the consequent unhealthiness of many sections.

Other causes of agricultural decadence and unhealthiness.

Other causes of agricultural decadence and of unhealthiness have manifestly been at work in northern Africa since the Arab conquest, such as the waste of manure, which is left to breed disease around the gourbis and douars, the want of proper alternation in crops, the superficial mode of tilling and the always incomplete cultivation of even the small surface that the Arab deems strictly necessary for the maintenance of his family and domestic animals, his calculations (?) being based on an average crop.

This is not true of the Kabyle mountaineers, a different and thrifty race, comparatively progressive and who, like the Swiss, cultivate in a primitive way, it is true, yet very generally, all their soil.

Urgent reasons for replanting of trees.

To sum up, of all countries, Algeria and Tunisia, so sparsely inhabited as compared even with the less densely populated nations of Europe, require to be well provided with wood on account of the general dryness of the climate (except on or near the seashore), the unequal distribution of the rain-fall, which occurs only during the cooler months of the year, when the heavy downpours are in a great measure wasted by the impermeable nature of the soil on the Tell, where the headwaters are torrents, and the lower and more level parts of the small rivers lose by evaporation much or sometimes even all of the water retained for any length of time in summer; woods are needed also on account of the great variations in temperature and dampness before mentioned, and which in the middle and southern zones produce nycthemeral differences of as much as forty and even fifty degrees (centigrade), while in the northern, the hygrometer attains its extreme recording limits, now under the influence of winds immediately laden with the moisture of the sea, anon subjected to the parching action of the desert. Not only should woods be protected in these colonies, but as many trees as possible should be grown; for is it not an axiom in climatology that (except in countries lying in the path of damp winds) a large proportion of woods is indispensable to that equable distribution of heat, cold and dampness which produces successful agriculture, a healthy climate and general prosperity?

II.

Chronological Facts as to the Growing of Eucalypti in Algeria and Tunisia.

Date of introduction of Eucalypti into northern Africa.

The first seeds of eucalypti consigned to the earth in northern Africa were sown in the Jardin d'Essai of Algiers in 1862, by Mr. Hardy, director of the botanical garden thus named, and in the same year by the Comte de Belleroche, who procured them from the director and sowed them in his property in the Commune of El Biar, four miles from town.*

Successive experiments in growing them as a preventive of malaria.

These experiments having succeeded, the trees were soon grown to prevent malaria, still so prevalent throughout northern Africa, and which made most cruel ravages in Algeria between 1867 and 1876, while immigration and the development of the colony were receiving their greatest impulse.

The importance of preserving the public health where satisfactory, and of improving it in the more numerous districts where conditions and circumstances were against it, was, at this time, more generally recognized by the government and the people. The "Ponts et Chaussées,"† the important companies and societies, corporations, municipalities and many private individuals grew eucalypti in the principal settlements infested by the disease, believing that they had at last discovered a panacea against the evil.

In 1868 Mr. Ernest Lambert, inspector of the forests of Algeria, sowed a grove on the Bouzareah mountain, above Algiers, where now is the forest, or rather wood of Baihnen. Then Dr. Marés, at Boufarik, planted a grove on his farm, reporting to the Société d'Agriculture seven years later that the health of his neighborhood was satisfactory. Malaria in its worst forms had constantly prevailed there until then *and until the land had been successfully drained.*

During the two succeeding years, the Société Algérienne planted 100,000 eucalypti near Ain-Mokra, a village on the shore of Lake Fetzara.

The mining company of the Mokta soon followed with many still larger plantations in the same region, where the public health improved towards 1875, the mines being thenceforth worked during the summer, an impossibility until then, owing to the excessive mortality among the workmen, due principally to pernicious forms of malaria.

The latter plantations remain among the most extensive in Algeria, and offer a striking instance of the frequently great aid given by eucalypti against malaria. Thick curtains of the trees were grown between the lake and the village, while, at the same time, a draining canal was cut in

* Now known as El-Afia, and belonging to the author.

† Government engineers, entrusted with the construction and repairing of roads and bridges, and the buoying of harbors.

the shallow bed of the lake, sufficiently deep and wide (so thought the engineers) to carry off the stagnant waters and dry up the swamp. This result, however, was not attained, but yearly thenceforth the waters of the lake were emptied early enough in the spring, and before the summer heats, for the spongy shores to be covered with an herbaceous vegetation offering here and there comparatively fair pasturage. The coincidence of this partial draining with the planting of eucalypti does not permit the conclusion that the improved sanitary condition of Ain-Mokra is wholly due to these trees.

At Maison Carrée, Cardinal Lavigerie and the white Fathers, as well as MMs. Sauliére, Cordier, Trottier and others sowed and planted, the first large the last small, groves of eucalypti, with a marked improvement on the health of the community, which, however, still remains far from good.

These enterprises were rapidly followed by many others, and now most Algerian villages, especially if in malarial districts, have more or less extensive groves or avenues of eucalypti, and many farms are also well provided with these trees.

III.

General and Special Advantages Claimed for Eucalypti. Limitations as to the Uses of and Objections to Them.

Advantages of trees in general, including eucalypti: shelter, good effect on the morale and on health.

Among the advantages of trees in general, shared to a certain extent by eucalypti, is the grateful shade procured in hot countries to dwellings, and to cattle, and other domestic animals.

Trees also, including eucalypti, gratify the eye, and the latter have totally changed the aspect of the plain of the Isser river, since they have been grown around its villages and farms. This is not merely an æsthetic result. The fact has its practical importance as acting directly on the morale and therefore indirectly on the physical state of the colonist. For trees form in the barren regions almost the only objects on which the eye rests with pleasure, recalling the triumph of man over desolate nature, diminishing in the heart of the pioneer that terrible longing for less stern realities and cherished scenes in the past, which, if not checked in time, opens the door to disease, even in the most robust constitutions.*

Forests cause the winds to ascend and produce rain.

Another general advantage of trees, particularly of eucalypti, is that forests, like mountains and other barriers, as is well known, when opposed to the wind force it to rise, dilate and cool in the higher and more rarified layers of the atmosphere, whence result con-

*We remark incidentally that in Algeria and Tunisia trees are not more numerous than at the time of the French conquest; they are fewer in fact. But trees, especially eucalypti, have been grown judiciously, where most serviceable to health. The cultivation of the vine has also accomplished much good, more even than eucalypti, because so much more extensively planted.

densation, saturation of the diffused aqueous vapors and finally rain. If, as Mignet says: "A forest is worth a mountain to produce rain," then the higher and more numerous the trees, as the higher and more extensive the mountain, the greater the precipitation of water, *ceteris paribus.** Scrub growths seem to exercise little or no influence on the rainfall, as witnessed in Greece, northern Africa and elsewhere, where this wild vegetation is principally composed of lentisci and dwarf palms, while we observe that the few million trees grown in Egypt under Mehemet Ali and his successors have brought back rains unknown for ages. This is doubtless a fair inference and not merely a coincidence due to other, such as cosmic causes.

Special advantages of eucalypti. Rapid growth.

Among the special advantages of eucalypti, one of the most important for the colonist, who can ill afford to wait long for a result from his labors, is their rapid growth, as compared to that of other trees suitable to this climate, excepting perhaps some acaciæ mimosæ, as shown by the following table approximately correct for an average appropriate soil and exposure:

Age, Years.	Height, Metres.	Circumference, Metre.
1	3	0.10
2	5	0.15
3	7	0.30
4	10	0.40
5	13	0.55
6	15	0.75
7	17	0.90
8	19	1.10
9	22	1.45
10	25	1.60

Resistance to heat and to slight frost.

Moreover the trees thrive where no others will, in the bad lands of these colonies, generally resisting great heat, and several species withstanding relative cold and even slight frosts and snows, as in Australia.

Antimiasmatic action.

To their balsamic odor† is perhaps due an antimiasmatic action on the surrounding atmosphere; and certainly the constant evaporation through their leaves of the dampness taken up by the roots is a most important agent of improvement for soils needing to be drained, while these

*Bare mountains lying in the path of damp winds naturally produce torrents and landslides instead of the useful rains occasioned by wooded mountains.

†This balsamic exhalation from the young shoots, twigs, the leaves and fruit is due to an essential oil similar to that of cajeput, which being oxidized by the air, produces ozone, and which, when refined, gives eucalyptol, a sort of camphor in composition and chemical properties, most serviceable as a febrifuge, tonic stimulant, aseptic and antiseptic.

trees have not the inconveniences of some other hardy trees of a slightly less rapid growth, but also useful for draining, such as plane trees, to which are ascribed (?) many cases of conjunctivitis and keratitis, prevalent in Algeria, Tunisia and throughout the East generally.*

Frequently malaria is not due to the soil on which a village or farm is built, but to the neighborhood. In this case a heavy curtain of eucalypti interposed is always useful and often sufficient to arrest the disease. Of course the swamp, or whatever be the nature of this infectious soil, must not be too extensive or pestilential, and the curtain must be of sufficient extent and thickness.

They form open forests.

The eucalypti form open forests, free from underbrush, that great temptation to the incendiary shepherd, who sacrifices health and well-being to a scant resource in actual pasturage for his flocks (see above the effect of burning down trees). If the subsoil be compact, the roots return to the surface; if permeable, they remain sometimes deep enough to allow a few scant and coarse grasses to grow between and under their shade, if the trees are far enough apart.

Their seeds prolific.

The seeds are light and fertile and readily disseminated by the wind, thus propagating their species and extending plantations.

A permanent benefit to the atmosphere.

The foliage is perennial; its benefit to the atmosphere (hygrometrically, electrically and antimiasmatically) is permanent.

They are killed with difficulty.

Besides, many species are killed with difficulty, and when destroyed above ground by axe or saw send out numerous shoots from the stump; at first easily broken off, but finally firmly fixed, and during the first three years or so giving leaves similar to those of young trees of their age; that is, lighter in color, more flexible, sticky, cordiform, etc., and possessed of greater antimiasmatic virtue than the leaves of older trees.

A great protection to land against wind.

It is well known that the protection of land against wind by an obstacle interposed between it and the wind is directly proportional to the height of the obstacle and approximately to twenty times that height. Therefore, eucalypti protect a much wider tract than most other trees against strong or otherwise harmful winds, such as the blighting sirocco. With a height of forty metres they protect a strip of four-fifths of a kilometre in width, the highest indigenous trees not protecting more than half this surface. The height to which eucalypti rapidly attain is, therefore, a sufficient reason for preferring them to other trees, except some Acaciæ, Mimosæ, to protect land against winds. Alternate rows of eucalypti of appropriate species can be judiciously cut down near the ground

* At Boufarik the great improvement in public health is due to plane trees, and mainly to the thorough draining of the marsh on which the village is built, and where hundreds of colonists lie buried.

and kept trimmed, so as to afford protection by the branches sprouting from their mutilated stumps against wind passing between them and the higher trunks of the rows left uncut.

Richness of their ashes in potash.

Finally, ashes of the eucalypti contain more potash than those of most European or North American trees.

A resource as firewood.

For kindling and firewood, as fully described further on, most eucalypti offer no advantages, although serviceable when other woods are scarce and dear, and constituting a precious resource in these colonies against the ever-increasing price of fuel.*

Necessary limitations to their climatological and hygienic advantages.

Naturally eucalypti have their limitations, as has every useful plant in nature, and it is a well-known fact that they have not materially improved the unfavorable conditions of disease-breeding soil and atmosphere in the oases, where the former remains undrained and indeed undrainable, except at the sacrifice of fruitful vegetation, lost as are these favored spots in the immense desert of ever-heated sands. Again, even the most extensive forests of eucalypti cannot neutralize the poison of very large swamps or of flatlands inundated only throughout the winter, as is the bottom land containing Lake Fetzara, already mentioned, where the trees cannot be planted with success, either on account of the excessive moisture of the ground or by reason of its brackishness resulting from the great evaporation.†

It would, indeed, be expecting too much from eucalypti to count upon their counteracting in Northern Africa all the evil influences at work in many parts, and which in other countries they have been vainly expected to overcome.

Italian reports conflicting.

The Italian reports are not generally favorable to eucalypti, nothing decisive, it seems, having been ascertained as to their superiority over all other trees in rendering less unhealthy the immense swamps of the Roman Campagna. It must be conceded that the climate of Italy is less favorable to these trees than that of the Algerian and Tunisian coast regions. However, as noted by De Pietra Santa,‡ "Malaria remains prevalent and

* Mr. E. Lambert, before quoted, claims other special advantages for the eucalypti, such as their immunity from the mandibles of the locusts, who devour other vegetation and even linen; and he mentions the protection their shade would afford to the thrush, black birds and other locust-eating birds if these trees were more extensively grown in the barren plains. He also claims that their foliage and bloom would feed the honey bee, as in Australia, whereas apiculture is now generally confined to the mountains, which are better wooded and less parched in summer, when, in spite of the heat, the insects remain active in this climate.

† "*Eu. rostrata* lives in water containing as much as 1 per cent. of chloride of sodium, but with as much as 1.50 per cent. good results are rare" (Dr. Trabut, Professor of Natural History at School of Medicine of Algiers).

‡ Pietra Santa, "Assainissement de la Campagne Romaine," *Journal d'Hygène*, 1881-1883. Also *Genie Civil*, May, 1883, Vol. iii, p. 312.

severe in the very districts of the Campagna of all others where it was expected that the disease would have been stamped out, so to speak, by the general planting of eucalypti, especially when, as was the case in many of these places, vigorous cultivation of the soil was added to their expected action."

Australian reports prove a limited antimiasmatic action against powerful causes of malaria.

If now we turn to Australian reports, we remark, as recognized years ago by Prof. Liversidge, of the University of Sidney, that: "Malaria is far from rare in the vast forests of eucalypti of Australia." Although without doubt these trees have always a beneficial action, this is not sufficient, as previously stated, to overcome the powerful causes of unhealthiness that are at work in many places. Referring to this point, Tomasi Crudeli* justly remarks that: "If all malarial soils had the same chemical composition and were similar topographically (and we may add if they had the same climate), then, perhaps, these trees could be expected to improve the unhealthy soils, so as greatly to attenuate or even to eradicate the disease, if at the same time all the diverse modes of improvement which have succeeded in rendering some of them healthy were applied; at least, we could only be justified under such circumstances in expecting a good result. Unfortunately, malaria is bred in very dissimilar soils, and we even recognize its presence on the granitic plateau of Castille. So that systems of soil improvement applicable to some malarial regions are useless in others. Until now we have proceeded empirically wherever we have introduced eucalypti, and such will be the case until a long series of scientific observations and researches, combined with practical experiments, shall have furnished exact information as to each distinct variety of soil which produces malarial poison."

If such be really the fact, let us trust that the dawn is breaking, and that each ray of light thrown on the subject even by such short papers as this (be the ray never so weak) may, when collected into a beam, aid us in seeing where the truth lies.

Objections to eucalypti as being ugly, as being deficient in shade, as twisting their fibre to the left, as not growing with other trees and as not being remunerative.

Objections have been and are still urged against eucalypti. We will only refer to them here, adding a word or two of refutation. This first objection is that they are ugly. This, however, is only relative, and does not extend to all species, some being quite ornamental. The second is that their leaves hang vertically and give incomplete protection against sun or rain. But such protection is preferable to none, surely. Another is their strong tendency to twist to the left,† which greatly interferes with their being sawed into

*Tomasi Crudeli, "La malaria de Rome et l'ancien drainage des collines Romaines." Lecrosnier, 1881.

†This levogyration, which constitutes the main objection to eucalypti, after the consideration that they are unremunerative, has never, as far as known, been explained satisfactorily. It is, however, much less manifested, as here noted, in close and extensive plantations, and there is a marked difference among the species as to twisting. But why is this twisting ever to the left, without regard to the direction of the wind?

planks; but this twisting can be lessened in many cases by growing the trees in close and extensive plantations, which gives most of them proper protection against the winds. Yet another objection is that eucalypti will not thrive generally when intergrown with other trees, and will interfere with the other trees and even kill them off; or, more rarely in these colonies, that they will be injured by the other trees. Both of these facts can be prevented by leaving sufficient space between eucalypti and the other trees.

The principal and insuperable objection to eucalypti requires also but a simple mention here: there is no money to be made from them, or, at least, such is the experience of the growers until now, the trees having been introduced into Algeria and Tunisia more than a quarter of a century ago. Those who recommended their being grown by others for a large profit have benefited by being prematurely rewarded by the government for their zeal.

Alas! that favorable prophecies, with all the calculations to support them, should have proved fallacious.

IV.

Species and Varieties of Eucalypti Most Serviceable in Tunisia and Algeria.

Among the very numerous species and varieties of eucalypti, our choice is founded on the recent study and actual knowledge of the trees. *Eu. globulus* (blue gum) grows well enough in generally dry soils,* and yet is especially suited to damp subsoils; its leaves and fruit are rich in essential oil and it is abundant in its indigenous soil, Australia. We owe the fact of its being the first species introduced into northern Africa to these advantages, as mentioned in our second chapter; also to the fact that there was at the time a relative, if not absolute, ignorance of the merits of the more valuable and equally hardy or even hardier species (which are still not sufficiently known in these colonies). But its wood is inferior for any purpose, as is fully stated elsewhere, and the red gums have been generally preferred within the last few years.

We refer at length in our last chapter to the many qualities of *Eu. marginata*, which is as yet so extremely rare as to be scarcely noticeable in a practical nomenclature of species found here.

Among the most remarkable species of red gums grown here are *Eu. rostrata* and *Eu. resinifera*, and numerous hybrids or crosses of these species. The former, when extensively grown from the seed and planted out, furnishes a good wood, withstands the dryness of the summer in the interior, seems to be one of the most resistant of trees, and reproduces itself spontaneously in the coast region (where, probably, it will soon be-

* Nevertheless it sometimes dies suddenly without apparent cause after attaining a considerable size.

come acclimated). The latter withstands intense drought and requires deep and dry soil.

Besides the above species, among the most robust and advantageous to northern Africa, according to Dr. Trabut, are the following: *

Eu. tereticornis.
Eu. amigdalina.
Eu. botryoides.
Eu. colossea (*Eu. diversicolor*).
Eu. cornuta.
Eu. corinocalyx (dry soils).
Eu. gomphocephala (still rare, but most useful).
Eu. goniocalyx.
Eu. leucoxylon (*Eu. sideroxylon*).
Eu. maculata.
Eu. mülleri.
Eu. occidentalis.
Eu. polyanthema (Shaw), *Eu. populnea* of Müller, *Eu. populifolia* of Hook, etc.
Eu. rostrata (brackish swamps).
Eu. robusta.
Eu. romeliana (hybrid from *Eu. botryoides* and *Eu. rostrata*, leafy and strong, obtained by Dr. Trabut).
Eu. rudis (large capsules).
Eu. soligna.
Eu. viminalis.

V.

Where, When and How to Grow Eucalypti in These Colonies.

Where to grow them.

Eucalypti, like *Acaciæ*, *Mimosæ*, and plane trees, thrive in countries where there are but two defined seasons; yet in Algeria and Tunisia they are only to be grown in the coast region, especially in the larger valleys and on the hillsides. Neither the extreme cold of winter on the high plateaux of the central zone, nor the extreme heat of the southern or Saharan zone and the changes between the temperatures of day and night, are suitable to them. Adaptable to widely different conditions of temperature, according to species and to the composition, depth, dryness or dampness of the soils in different parts, yet, in the words of Sir Lambert Playfair,† it would be

* Dr. Trabut, Professor of Botany at the Ecôles Superieures, Algiers.

† Sir Lambert Playfair, Consul General of Great Britain at Algiers, Report on the planting of Eucalypti in Algeria, May 16, 1877, No. 21.

"as useless to attempt to grow them in the Tropics as it would be in the north of Scotland."

For species suitable to special soils see preceding chapter. Generally speaking, eucalypti should be grown throughout Algeria and Tunisia, preferably in swampy localities, on the shores of lakes, around ponds either shallow or brackish and partly dry in summer, in damp bottom lands, on the banks of water courses which are sluggish or frequently changing their beds (as are most north African rivers, which often ruin whole valleys that might be fertile under other conditions), in places exposed to landslides or slips, for they are generally not on a large scale, although frequent on account of the abundant clay of the coast region. We have seen also that they aid in protecting villages and farms against noxious winds, sun and the malaria, whether bred *in locis* or in the neighborhood. Finally eucalypti are advantageously grown in any appropriate soil of little value for other purposes, if a judicious choice be made among the species. Whatever be the locality chosen, the surface soil must be permeable and otherwise suitable; the subsoils, if compact, force the roots to spread out mesh-like to considerable distances, sixty metres as we have measured, in the direction of water or of deeper and better or damper soil.

Where not to grow them.

Without a ditch of a couple of metres in depth being dug as a separation between eucalypti and the other more valuable plant, no eucalypti, particularly not *Eu. globulus*, should be grown near these plants (orange or other fruit trees, vines, flower beds, etc.), nor too close to a spring (always most precious in these colonies), a well, a reservoir, a building or any useful wall, as eucalypti send out roots which absorb the nourishment of other plants, and sometimes ruin constructions even of cement.

Three modes of propagation.

Eucalypti are grown from seed, either sown *in loco*, in the open field where the trees are to remain, or, preferably in Algeria and Tunisia, the seed should be sown in pans, the young trees being planted out properly and at the proper time; or they are grown from young trees.

The seeds take from fifteen to twenty days to germinate, according to soil and season. They are small, light and generally fertile. They should nowhere be covered by more than a centimetre of finely divided earth.

Water is generally scarce in Algeria and Tunisia, and artificial irrigation being expensive, cannot be attempted, if the plants are to be grown on a large scale.

Preparation of the ground for sowing and planting.

For both sowing and planting, the ground should be prepared several months before the seeds or the trees are consigned to it. The soil should be broken up by a subsoil plough to a depth of 0.05 metre or more, when possible, and all foreign growths removed.

Sowing in the open.

Shortly before sowing in the open, the ground should be ploughed crosswise, that is in both directions, and reploughed lightly in furrows 1.5 metres apart. The seeds should be carefully deposited every two steps (or at intervals of 1.5 metres) and covered with a thin layer of fine earth. Of course, this entails irregularities in the interspacing of the shoots, as many seeds do not germinate, being blown or washed away or washed under, and the young plants of the same species grow more or less rapidly, according to the quality of the surface soil, and in a lesser degree to the nature of the subsoil in various places in the same localities, and moreover the growth is less rapid for some time than when young shoots are planted. This sowing in the open, which should take place at the beginning of the rainy season, appears to be cheaper than sowing in pans and planting out the young trees a few months old, the labor being so much less, but in the end it is dearer as so many seeds do not germinate, and the sowing has to be renewed frequently.

Sowing in pans to plant out the shoots.

The seeds are preferably sown in pans or boxes, and the young trees planted out at the proper age and season.

"Prepare a compost of vegetable mould and river sand very finely sifted. Fill the pots of 0.15 metre in diameter, press the earth lightly and evenly with a small zinc cylinder of about the same diameter as the pot. Scatter the seed on the surface so as nearly to cover the whole of it, then, with a very fine sieve, which may be a zinc cylinder similar to the other but perforated with very minute holes, sift just enough of the compost on the seed to cover them and no more. Press this surface again lightly with the first cylinder and water with a watering pot, the rose of which is perforated with the smallest holes which it is possible to make. This should be done in early May, so that the trees may be planted out at the first rains of autumn when the ground is moist. Within fifteen or twenty days the seeds will have germinated, and in about six weeks the plants will be ready to put out. Weed off as soon as the trees have produced four leaves, and transfer to other pans of 0.1 metre in diameter, to be kept in a shady place for the first day or two, and then transfer to a sunny position; water during the summer just sufficiently to prevent them from dying. The great object is to retard their growth during the summer so as to keep them small and prevent their roots from becoming matted inside of the pans.

A second sowing may take place about the middle of September, so as to obtain young plants ready to be put into the ground about the beginning of spring. In some respects this plan is preferable to the other, and it is always so when the plants can be watered in summer. The young trees have a shorter time to remain in the pans, and their roots run less chance of becoming matted; but often, when the rains cease early in the year, they have not become sufficiently rooted in the open

to enable them to resist the heat of summer without occasional irrigation.

"The Eucalyptus is a plant that does not stand being kept long in a pan; its roots grow with as great rapidity as the rest of the tree, and, if they are allowed to be contorted round the inside of the pan, the tree does not recover from this unnatural condition of things and seldom grows straight and healthy."

Planting in the open on a large scale.

As previously stated, as soon as the ground (which has been broken up and freed from other growths, late in the winter or early in the spring while wet) becomes again impregnated with the rains of autumn, plough and plant out the young trees of three to five months' growth (which have often five to eight leaves each), at interspaces of four metres in trenches, and as they increase in height, progressively fill in the trenches, till in six months they have entirely disappeared, and instead of a depression, the earth becomes piled up round the stem of the young trees; this serves not only to keep the roots moist, but to prevent the slender stem from being blown over by heavy winds against which eucalyptus should always be protected as much as possible to prevent twisting and a slow growth.

It is well to give each plant a good watering when put into the ground, but they will generally not require another (?).* The soil should be kept free from weeds and open for the first two or three years, which may be conveniently done by passing a cultivator between them in each direction once or twice a year. After the third year they may be left to themselves and will require no further care.

"Weakly specimens are eliminated wherever necessary and their places filled with hardy plants, until a full plantation of trees is obtained from four to five metres apart."

Planting on a small scale.

When eucalypti are to be planted on quite a small scale, instead of trenches, holes of a cubic capacity of 0.5 metre may be made; but this is not to be recommended in the open field, as the heavy rains are apt to fill up the holes with earth and smother the plant, instead of being carried off by the open trenches above described.

Definite aspect of a plantation.

"By judicious management plantations can be obtained in which the trees are about four metres apart, and after ten years or so, every alternate row in its entirety may be cut down, leaving the remaining trees at eight metres apart."

* It sometimes happens, when the rains cease early in the year, that the young eucalypti have not become sufficiently rooted during their short sojourn in the open ground to enable them to resist the heat of summer without occasional irrigation.

VI.

Commercial Value of Eucalypti in Algeria and Tunisia.

Pecuniary profit from the trees generally.

The retail price of *Eu. globulus*, much the most abundant among eucalypti in Algeria, when cut up for fuel and sold in Algiers is $0.50 a quintal (100 kilogrammes = 220 pounds), and yet we have been offered by the trade for full-grown trees the same sum, all expenses of cutting down, sawing and splitting into hearth-logs, as well as of carting to town being assumed by the buyer.* The road to town is good, down hill and only four miles long, and the cost of transportation is estimated at about ten cents a quintal. If the road to market is not very short and good, the trade will not buy standing eucalypti at any price, as there is no profit, and frequently a positive loss in the transaction, and in the immediate proximity to any good market the purchaser has to pay too high a price for his land to grow eucalypti for sale.

Thus we see that the business scarcely exists at all on any scale worth a longer notice here. And yet firewood is generally wretched in the coast region, good wood being procurable only in the mountains where, with the exception of the several military roads which are admirable, the roads are few and bad. All fuel is therefore relatively dear, because until now no coal mines have been worked, although several are said to exist in the colonies.

In the towns and even in Algiers old boxes, rafters from torn-down houses and ragged roots of lentiscus are offered and bought as fire wood.

Details as to expenses of growing the trees, and margin of profit or loss.

Counting 800 trees to the hectare (2 acres 1 rood 35 perches) left after ten or twelve years, if the trees are then marketable (as they rarely are under the most favorable conditions and circumstances), we have, at say 50 cents each, $200 for the product of an hectare for ten years, or $20 a year, that is, about $9 a year for the acre. From this sum, if we subtract the cost of growing the trees in the most economical way, which is one-twentieth if the trees are grown from the seed planted *in loco* (as previously noted), and which may be estimated at $4 a year per acre; and the interest on the price of the land and other incidental expenses, we find no profit left, or even a pecuniary loss, unless we start with very cheap or free land, most favorably and exceptionally well situated and with 2000 trees per acre, to be weeded out during the first five or six years: and unless we can sell these younger trees, which is a very rare occurrence, the trades preferring other woods

*These trees were thirty years old, but under the most favorable conditions the trees would possibly have brought the same price at fifteen.

for the numerous uses to which eucalypti are put in Australia, doubtless for want of better wood.*

A small market for the accessory products of eucalypti.

Not only is there, generally, no profitable market for the wood of eucalypti in Algeria and Tunisia, but there is a very small sale of the undoubtedly important accessory products of these trees.

From the leaves, twigs and fruits, giving the essential oil, there is still a little profit. For the oil when produced by the colonist the demand is relatively small compared to what it should be, high prices being asked for it by the retail dealers.† As to the tannin, it is not used in Algeria and Tunisia, nor in France, as it is in Spain and Portugal for the tanning of leather; while the tannin of Mimosæ, or mimotannic acid, is recognized as a most efficient aseptic and antiseptic, rendering valuable services in therapeutics, and successfully used in diphtheria by Dr. Bourlier, the discoverer, and others, as prepared by C. Brenta, of Algiers.

Possibility of some choice species being peculiarly profitable in the future.

Perhaps some choice species of eucalypti, such as *Eu. marginata*, *Eu. leucoxylon* (the *black* variety), will redeem the reputation of the trees as a source of pecuniary profit, when grown under the most favorable conditions and circumstances. Until now, however, nothing worth recording has been accomplished with this

* In Australia, as stated by M. Ernest Lambert, ex-Inspector of Forests in Algeria, eucalypti are in general use for manufacturing such implements as pitchforks from young trees two years old, whip handles, the handles of spades, hoes, sledgehammers and other articles of daily use. Three-pronged pitchforks, always relatively dear, are readily procured from the young trees, the stem of which is broken off or cut off and the leaves of which are stripped from the two side branches of such trees, or a branch is pinched so as to distribute the sap as regularly as possible in the three forks thus obtained. At three and four years old the trees make carriage poles or shafts, ladder poles, fence poles and rails, wheel spokes and other articles too numerous to be recapitulated here. At five years telegraph poles are obtained, which the above-named author and others affirm to be more durable than pine poles, and not to need to be injected by a preservative substance to enable them to last. The pine poles are only procurable from trees of twenty-five years' growth, during which, say the above-mentioned authors, eucalypti give five fine poles to one tree. For supports in mines eucalypti have also their places well defined, as, indeed, for railway sleepers, five or six of which are to be had from trees of seven or eight years of age. At nine they serve as piles for docks and quays. When cut up at this age they are serviceable for wheel naves, carriage brakes and drays and what not, according to the same panegyrists.

† This oil is worth about $3 a kilogramme at Grasse, France. The parts of eucalypti employed in its manufacture yield 2 per cent. in weight, while 10,000 kilogrammes of the petals of roses and 700 kilogrammes of those of geranium yield but a kilogramme of these more valuable oils.

Many products, of doubtful origin, actually used in perfumery under fanciful names, of supposed Japanese and other origins, seem to have no other merit (when they are not positively offensive to the sense of smell) than their supposed scarcity and consequent expensiveness. Eucalyptol, if rare or still supposed to be, would doubtless be sought by the extravagant public as an agreeable exotic perfume. It would have the merit of being a clean product of great virtue for the toilette, which is more than can be said of any of these so-called perfumes—and, united in due proportions with pure white vaseline and good toilette soap, it should be extensively used for toilet purposes.

species, excepting the interesting experiment made by Dr. Bourlier on his farm near Reghaia, where a few of this species have been successfully grown, conjointly with divers Acaciæ and especially with Mimosæ.

Desirability of the propagation of such fine species as *Eu. marginata.*

A fine, strong, inexpensive wood, almost uninflammable and resisting decay, fit for barn and ship building, railway ties, piles, telegraph poles, paving blocks, flood-gates, carpenter's work and even cabinetmaking, such as *Eu. marginata*, the yarrah wood or Australian mahogany is claimed to be, would indeed be a boon to Algeria and Tunisia, which have so far not been blessed with any such treasure.*

Alleged blindness of the public and market to the merits of eucalypti in general.

It is a pity that, with the exceptions mentioned in this paper, the very many merits claimed years ago, and still claimed by some, for eucalypti in general, should remain unrecognized in Algeria and Tunisia by those who have been induced to make the experiment of growing eucalypti for profit. Either the public and the market are blind to the merits of eucalypti, or else the numerous services rendered by these trees are still better rendered by others at present in use for agricultural and industrial purposes, as well as for fuel. It is needless to mention which of these suppositions is the most likely.

On the Remains of the Foreigners Discovered in Egypt by Mr. Flinders-Petrie, 1895, now in the Museum of the University of Pennsylvania.

By Mrs. Cornelius Stevenson.

(*Read before the American Philosophical Society, March 20, 1896.*)

Before entering upon my subject, I must explain that what information I have with regard to this remarkable collection is mainly derived from private letters received from Mr. Flinders-Petrie last winter at the time of this most brilliant of all his brilliant discoveries, and at intervals since then. Very little has, as yet, been published concerning them. The

*Like the reed of the fable, *Eu. marginata* is flexible and bends readily without breaking. A block of 0.5 metre in length and offering a square section of 0.25 metre bears, before breaking, a weight of 1400 kilogrammes suspended from its middle, 900 kilogrammes being the breaking weight of a ruler of oak of the same dimensions. The resistance of *Eu. marginata* to crushing in the same condition is also greater than that of oak (both woods having the same density), and is 350 kilogrammes to the square centimetre of bearing surface; its tensile strength is remarkable, 890 kilogrammes to the square centimetre. Its resistance to parasites is very great, even the terrible white ant cannot perforate its grain, nor does the *Teredo navalis* cause its prompt destruction, as is the case with other woods used in naval constructions, for *Eu. marginata* has been known to withstand the action of the ship worm for thirty and forty years (E. Lambert, above quoted).

report has not yet appeared, and the only sources of information available are a catalogue of the objects exhibited at University College in London last July; some short articles published by Mr. Flinders-Petrie in the *Times* and in the *London Academy* and reproduced in the *American Journal of Archæology*, and a leaflet issued by the "Egyptian Research Account" as a brief preliminary report to its subscribers. These with the private letters above referred to form the basis of this paper.

You are aware that last winter Mr. Flinders-Petrie, whilst working in the neighborhood of the villages of Ballas and Nagada—that is some thirty miles north of Thebes (near the twenty-sixth parallel) on the western bank of the river and on the edge of the desert—made some remarkable discoveries.

In this locality were some Mastaba-tombs of the old empire (IVth to VIth dynasties) and a Mastaba-like pyramid, similar in form to that of Sakkara, with a sepulchral chamber scooped out of the sand-bed below, but entirely constructed of natural blocks, selected for size, and in no way tooled or even broken, and therefore probably one of the earliest of such structures.

The Mastaba-tombs likewise offered interesting peculiarities: access to them was obtained through a stepped passage, which sloped down from the north as in a pyramid. Nearly all these tombs had been anciently plundered, and little, save a large number of stone and alabaster vases, was found belonging to their original occupants.

In some of these ancient tombs, however, were discovered burials of strange intruders, the evidences of whose general culture, beliefs and funeral customs show them to have been strangers in the Nile valley. Not a single detail of their culture did they hold in common with the Egyptians. Moreover, their number, which was found to have spread over a considerable portion of upper Egypt, from Abydos to Gebelen, over one hundred miles, whilst their influence was observable from Tenneh to Hieraconpolis, *i. e.*, over three hundred and fifty miles, and the absolute control of the region which they assumed and which is shown by the total absence of any object recalling Egyptian civilization, show them not only to have been invaders, but invaders who once had swept over the region and who, settling down, had lived there for a considerable period, borrowing little or nothing of the people whose land they occupied. As Mr. Petrie wrote in the first outburst of enthusiasm following upon his great discovery: They form "a grand new puzzle and might as well have been found in Siberia or in France for aught of their connection with regular Egyptian antiquities."

This complete wiping out for a time of the Egyptian civilization is one of the most striking features of this remarkable episode, and gives point to Mr. Flinders-Petrie's discovery. In the large number of burials opened "not a god, not a scarab, not a hieroglyph, not an amulet, not an Egyptian bead was found." These people were great pottery manufacturers, and yet, although they settled in a land where the potter's wheel had long

been in common use, all their pottery is hand-made and of form and decoration peculiar to themselves.

An Egyptian town in the immediate neighborhood yielded—in different strata—pottery of the IVth, XIIth, XVIIIth and XIXth dynasties, and presented not one single link with the peculiar manufactures of the intruders. What, then, had become of the Egyptians on this extensive tract of territory and during the considerable period represented by the layers containing variations of the original industries of the invaders?

The kings of the Vth dynasty who ruled over united Egypt were said by Manetho to have come from Elephantine, and vestiges of their power and of that of their successors (VIth dyn.) have been found from the southern frontier of Egypt to the peninsula of Sinai. Even recently, fragments of papyri have been found at Elephantine bearing the names of Rameri and of Noferkara which must be added to the weight of evidence already gathered to show the extent of their empire (London *Acad.*, March 14, 1896). They were powerful monarchs, and, like all of Egypt's strong rulers, they were active in their building enterprises and have left, written on stone, eloquent testimony of their power.

Of their successors, the Memphite kings of the VIIth and VIIIth dynasties, however, nothing remains save a few scarabs bearing names that can be identified with some of those given in the Egyptian lists for that obscure period. Indeed the silence of the monuments is so complete as to become positively eloquent. It is evident that some national catastrophy occurred about that time which caused the dismemberment of the great empire of the pyramid builders and reduced the power of their Memphite successors to comparative insignificance.

Manetho gives five kings for the VIIth Memphite dynasty and twenty-seven for the VIIIth. The Turin fragments give eighteen, and the tablets of Abydos give a selection of fifteen. No doubt can exist, therefore, as to their reigns having occupied a considerable period of time. There is evidence that during the IXth and Xth Herakleopolitan dynasties, Upper Egypt, which—as far as the monumental evidence is concerned—seemed to have been wiped out of existence, reappeared upon the scene of history, and that the princes of Thebes began to assert themselves and to grow in power. Some important inscriptions found by Mr. Griffith in the tombs of the feudal princes of Sîût cast a flash of useful light upon this obscure period. These princes, loyal to the kings of the Herakleopolitan dynasty, fought on their side in their wars against the Theban princes, whose increasing pretensions threatened the power of their liege lords. These facts are now all-important in restricting the limits in which must be placed the episode of the foreign intrusion just brought to light by Mr. Petrie's genius. It seems obvious that such an intrusion could not have taken place had the Theban princes been as powerful as they appear to have been under the IXth and Xth dynasties.

That the foreigners entered Upper Egypt after the great period of the pyramid builders is shown by the fact that the Mastaba-tombs referred to

above were usurped by them to bury their own dead. Moreover, in the step-passage of a Mastaba, a burial of the XIIth dynasty was found superimposed upon the remains of the strangers. Here were therefore three well-defined epoch-marking layers, and the fact that brick tombs of the XIIth dynasty were constructed over the ruins of a town occupied by these people, conclusively proves that their presence in Egypt preceded the Middle empire.

Four necropoles and two mud-brick towns extending over an area of five miles yielded the same result as to strata and relative occupancy. It is therefore reasonable to see in this intrusion of a strange race, spreading over so considerable a portion of the Egyptian territory, which it held for so long a period of time exactly coinciding with the monumental break in Egyptian history, if not the explanation of at least an important fact connected with that break; and to venture upon the assertion that a migratory movement of some magnitude took place about 3400 B.C., of which the people whose remains have just come to light formed a portion, and by which the first united Egyptian empire was weakened and brought to an end.

Mr. Petrie, assisted by Mr. Duncan, pursued his investigations at Nagada, whilst Mr. Quibell, working for the "Egyptian Research Account," explored the burials near Ballas, both exploring parties continuing their researches until over 2000 burials were opened and their contents examined and secured. These made it evident that the invaders had long retained their peculiar customs and beliefs: Instead of cutting their tombs in the solid rock as did the Egyptians, they dug their graves in shoals of gravel in the dry water courses of the desert edge; these graves are open square pits of the type of those found at Mycenæ; they were roofed over with wood, and their average dimensions are about 6 x 4 and 5 feet in depth. Their size varies, however, from half to double those here mentioned. Unlike the Egyptians who mummified their dead and laid them stiffly stretched out upon their backs, the body, reduced to a skeleton, here lay in a contracted position turned upon its left side, facing the west, with the head to the south. Every body, or ninety-nine out of a hundred, was found with the head taken off or removed. Short, oblong coffins of coarse pottery, with a lid and resembling a chest, were used. The bodies showed evidence of having been mutilated before burial. In one fine tomb, the bones were heaped in the centre, whilst other bones, the ends of which had been broken off and scooped out as though for marrow, were placed around them. This led Mr. Petrie to suggest that they must have been ceremonial cannibals. In other graves the bones were separated and sorted out.

Large bowls of coarse pottery, such as those exhibited with the coffin, contained ashes, probably of the funeral feast, and Mr. Petrie aptly quotes with reference to this custom 2 Chron. xvi. 14, xxi. 19, and Jeremiah xxxiv. 5, referring to a great burning made at every funeral—a custom probably Amorite. These were placed at the foot, and other jars,

such as may here be seen, and which originally contained liquids—beer, water, etc.—were placed along the sides. As many as eighty vases have been found in one grave, and few interments were provided with less than ten or a dozen. Among these were sometimes found a vase of black incised ware, evidently imported.

Jars of pottery with wavy handles, containing scented fat or its Nile-mud substitute, were placed along the head end, with a rough pointed brown jar in the middle. This type of pottery, which was very common and which gave rise to varieties of forms and uses during the sojourn of these people in the Nile valley, must be regarded as part of their industrial equipment, and is so specialized as to have led Mr. Petrie to suggest that these men were related to the Amorites of Palestine, who used similar pottery and who, he thought, might be another branch of the stock to which these invaders of Egypt belonged.

In bringing these objects to your notice, I am laboring under serious disadvantages and I must claim your indulgence should it so happen that I cannot make all points of detail clear to you. Although the collection reached here early in the winter, lack of proper space to work it up and to display it with safety, prevented my unpacking it until now, and I have not had a chance to study each specimen as it should be studied. This is all the more to be regretted as the material is quite new, and as, for the first time in the course of our much *more* than satisfactory relations, Mr. Flinders-Petrie, owing to pressure of business, was unable personally to superintend the packing, so that I have had very little to guide me in my identifications save my own limited experience and the general indications furnished in Mr. Petrie's letters. The types peculiar to these strangers are, however, as a rule readily recognized.

The main difficulty has been with the alabaster and stone vessels, of which we have a great quantity. These are principally derived from the Mastaba tombs of the old empire, and in sorting them there lies therefore some danger of confusion, especially where, as in the later layers of the invaders, a certain overlapping took place. I have, however, only brought here those specimens of Libyan stone work as to the origin of which I can entertain no doubt: Elongated vases of various dimensions with useless ledge-like feet too small for use, intended to be suspended by means of long tubular handles, a frog of breccia, and various other types which have no Egyptian equivalents. These stone vessels are hand-worked and show no trace of the turning-lathe. The material which I have not been able to determine with certainty must remain until Mr. Petrie's full illustrated report is published, when each group of objects in our collection will, no doubt, find its proper place.

Most of the flint implements now before you are from the invaders—these are oval in shape and equally worked on both sides. There are, however, a few dark weathered flints found upon the top of the limestone plateau, some 1400 feet above the Nile—all of which show signs of a longer exposure than that to which were subjected those flints to which

we know can be assigned more than 5000 years of existence under similar conditions. These are regarded by their discoverer as Palæolithic ; among them are two whitened flints of the pointed type, thickly patinated, also regarded by Mr. Petrie as Palæolithic.

The stone work of these people was, as may be seen, of the very highest order. We have here some flint bangles, one of which is perfectly cut to less than the eighth of an inch in diameter. Some of the finest blades excel not only anything done in that line by the Egyptians, but are unsurpassed by any ancient neolithic workmen. The exquisite regularity of the surface flaking and the fine serrated edge of some of their tools is startling in its perfection. Some forked stone lances used in hunting the gazelle are both curious and beautifully executed, and their numbers show their owners to have been great huntsmen.

It is more than probable that some fine specimens of similar workmanship found in Egypt from time to time and which have been brought into various museums were, in reality, relics of these people. Mr. Petrie has already called attention to a fine blade belonging to General Pitt-Rivers' collection and which is set in a handle of undoubted Egyptian manufacture. This is certainly the adaptation of an older blade.

These interlopers also used copper tools. Other metals such as gold, silver and lead were apparently known to them, although valued as rare products.

In their pottery they seem to have often aimed at reproducing the stone forms common among them, and even at imitating the very substance, such for instance as the limestone breccia, which they copied in splashed pottery, of which we have here a beautiful specimen.

The red polished and the black and red polished wares are the most common manufactures. Animal forms and curious devices were produced. The black and red is very distinctive. This is of the same material as the plain red, but is harder and is given a higher polish. The forms also differ, and are generally remarkable for the elegance of their proportions. According to Mr. Petrie, the black color is due to the "deoxidizing action of the wood ashes in the kiln, reducing the red peroxide to a black magnetic oxide of iron. The brilliant lustre of the black is probably due to the solvent action of carbonyl, due to imperfect combustion, which enables the magnetic oxide to rearrange in a continuous surface."

The effect of this process seems identical with that observed on certain vessels found by Dr. Richter in the lowest stratum of the copper-bronze age in Cyprus and approximately placed by him sometime between 4000 and 3000 B.C. In the collection which we purchased from him some years ago and which contains a part of the results of his own excavations in Cyprus, there is a round bowl to which the above date is assigned, and which is identical in coloring, polish and general effect to this black and red ware ; the form, however, is different from that of any vessel in this collection, and a small perforated handle for suspension on one side would in itself draw attention to a difference in the manufacture. It

would seem from this, however, that the deoxidizing process as systematically applied to red pottery for purposes of decoration was a widespread fashion at that remote period.

Some of the pottery of these strangers was decorated with crude figures of ostriches, antelopes, etc., often represented in long lines, in brown on buff and in red upon a lighter red. A very common decorative motive is a long boat with two cabins, an ensign pole and many oars; sometimes the figure of a man is added. The red polished ware, decorated in white lines, "dents de loup," plants and flowers, etc., is imported from the Mediterranean region. It is stated by Mr. Petrie to occur only in a limited range of the territory occupied by the foreigners, and it gave rise to no varieties of type. The shapes of these vases are also peculiar, especially the specimens in which two or three tall, straight stems or necks arise from one base.

The black incised bowls, with white decoration, in lines and "dents de loup," are also imported. No such pottery is known of Egyptian make, although in later times, during the Middle empire, a style of pottery similar, though much finer, appears. A near approach to it is found in the later Neolithic stations of Italy, Spain and in the lower strata of Hissarlik.

In a paper read before the Anthropological Section of the British Association—a notice of which was published in the *Academy* (September 28, 1895) and in *L'Anthropologie* (October–December, 1895, p. 590)—mention is made of a Neolithic station near Butmir, in Bosnia, recently studied and described by Mr. Radminsky, where pottery was found offering a great variety of decoration, among which, by the way, appears a spiral ornament. Figurines showing some artistic aspirations were also recovered. In the discussion that followed Mr. John Evans expressed the opinion that this station probably belonged to the transition period from the Neolithic to the bronze age. Certain holes cut in the clay reminded Mr. Petrie, who was present, of the sand pits dug in Egypt. He said that the pieces of black pottery exhibited by Mr. Radminsky were absolutely identical with pieces found by himself in Egypt and by others at Hissarlik and in Spain, and that he, therefore, would date such a settlement, by this black pottery, from 3300 to 3000 B.C., when it was generally manufactured (*Anthrop.*, October–December, 1895, p. 560).

Among the small objects in our collection are a number of bone combs and tools, one of which, a puncher, has just been identified by Prof. Cope as the metatarsal of a gazelle. We have also a series of slate pallets upon which Malachite, etc., was ground probably for tattooing purposes. These are in the shape of the turtle and fish, besides more simple forms, such as squares and rhombs ; but a larger variety of animal forms has been found, and Mr. Petrie mentions the ibex, elephant and birds among those in his collection.

It is worthy of notice that the taste for symmetry, which prompted the introduction of the double-headed bird design among so many ancient and modern peoples, was already developed among these men, as may be

seen by the handle of a bone implement. Here, however, the double-headed bird is no eagle but an ostrich.

Where was the centre of this culture—whence did these men come into the Nile valley? This must now be the problem which archæologists have to solve. It is the last riddle propounded by the Egyptian Sphinx.

They were a tall, robust race, with strongly marked features and a hooked nose. They wore a long pointed beard and had brown wavy hair, as shown by their representations of the human figure. Altogether they closely approached the type of the Libyans and the Amorites, and probably belonged to the same stock. Mr. Flinders-Petrie calls them Libyans, and Messrs. Evans and Boyd Dawkins corroborate this opinion. By Libyan here is meant a people inhabiting some as yet undetermined region of northern Africa, and representing a branch of the Neolithic culture of southern Europe, although these particular Libyans were just emerging from the Neolithic stage when they invaded Egypt.

The connections which can, through them, be traced with the contemporary Mediterranean civilization are of immense value. Not only do their importations from the Mediterranean region give us interesting glimpses of the active intercourse of nations *inter se* in those early days and reveal it to us as much the same in character and degree as it appears in subsequent ages, but they furnish us with the means of approximately dating certain typical Mediterranean products. As we find these associated in the Mediterranean region with the transition period of the Neolithic culture, it seems that we are more or less safe in regarding 3500 as the likely period of the introduction of metals into the western Mediterranean region.

Not only has Mr. Petrie's splendid discovery filled up what has long seemed a hopeless blank in Egyptian history, but it has furnished science with a solid foundation upon which the prehistoric period of Europe may stand whilst like a coral reef it builds its way up in an effort to reach the surface of history.

Before closing my remarks, I beg to take advantage of this opportunity to acknowledge Mr. Flinders-Petrie's disinterested kindness and liberality in helping us to develop in this city a museum which must prove an educational instrument of the highest value to our people. At a time when we are indebted to him for this priceless collection, it is but proper I think to publicly recognize the constant interest which Mr. Petrie has shown in our effort.

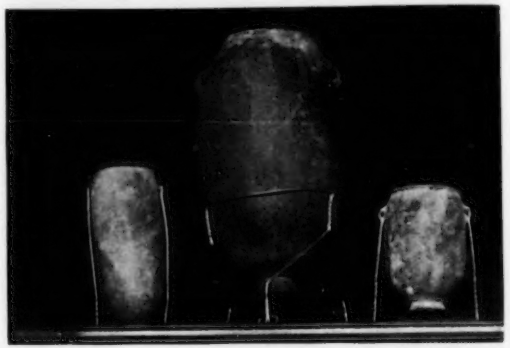
DR. D. G. BRINTON SAID:

The chipped flints which have been exhibited appear to be of widely different ages, those from the tombs showing scarcely any patina, while the two from the surface of the plateau are covered with a thick, white, weather-wearing. Of course, allowance must be made for the constant

exposure of the latter and the protected condition of the former. But this is not sufficient to account for the marked differences. Moreover, the shape of the plateau implements is distinctly "palæolithic." They are not intended to be hafted, but to be held in the hand when in use. What is further noteworthy about them is that obviously both are adapted to be held in the *left* hand only. So far as they go, they support the theory advanced by some writers that primitive man was less right-handed than later generations.

The pottery and stone articles from the tombs of the so-called "new race" near Abydos are good examples of their arts. I speak of this with some knowledge, as early last August I examined with much care Prof. Flinders-Petrie's immense collection in London, and had the advantage of his personal explanations. The article that I published in reference to it, in *Science* (August, 1895), was I believe the first original report on the subject in any American periodical. That the "new race" was supposed by Prof. Petrie to be Libyan, that is, Berber, attracted me, as the ethnography of that stock has been a special study with me.

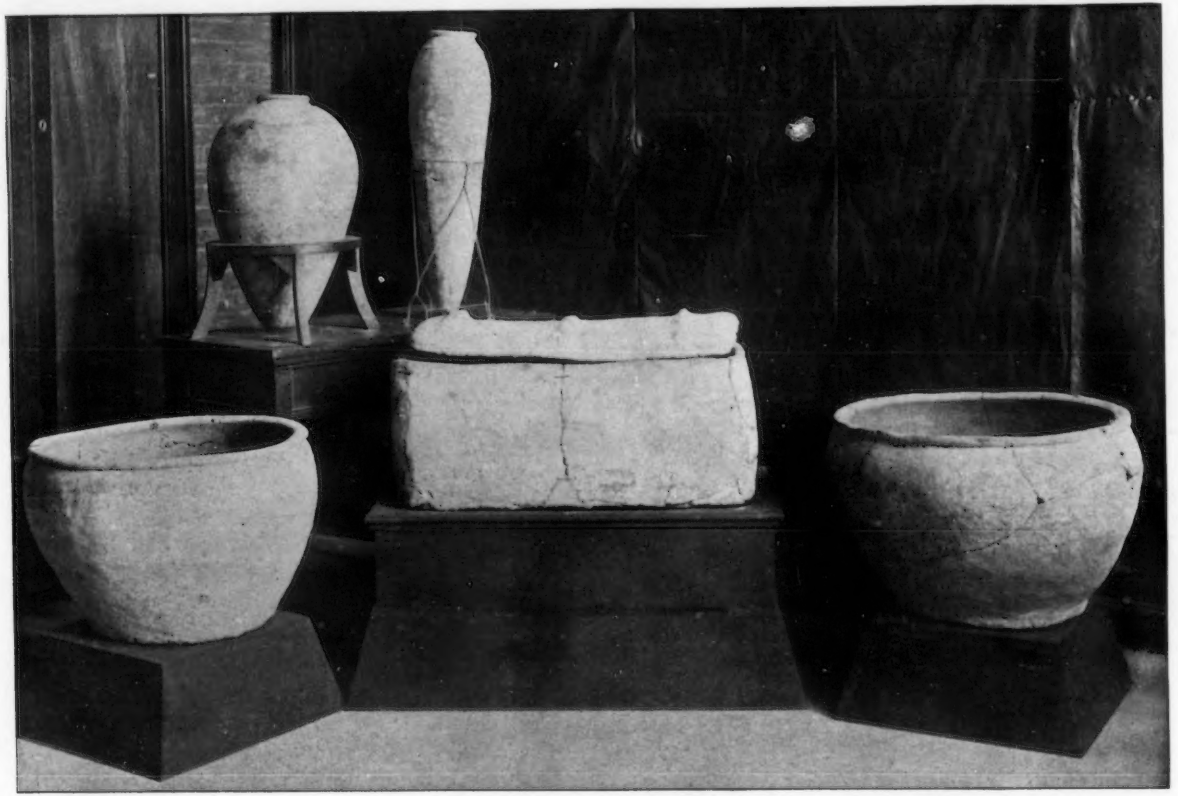
This identification, I believe, will finally be established. If we examine the configuration of the Nile valley and its surroundings, no other theory is tenable, providing the Libyan stock extended that far south of the Mediterranean at a date 3000 B.C. We know they did, and much earlier, from their very early presence in east Africa. The invading "new race" could not have come from the east. The natural highways from the Red Sea to that portion of the Nile valley centre at Koptos, and there few or no specimens of this peculiar art have been exhumed. They must necessarily have entered from the west, and a study of the ancient and modern caravan routes leads inevitably to the conclusion that their last previous station must have been the so-called "Oasis magna" of the Libyan desert. This consists of a series of arable depressions in the calcareous Libyan plateau, which here rises to an average height of about 1200 feet. The central portion of the Oasis is about 130 miles westerly from Abydos, and to it a number of caravan routes converge from the north, south and west. So far as history, archæology and linguistics teach us, this group of cases, as well as the "Oasis parva," opposite the Fayoum, and that of Jupiter Ammon, still farther north, have always been peopled by the Libyans. This stock has not been shown to be connected in culture with the Neolithic peoples of western Europe, and no positive traces of the Berber language remain there, though it is probable that the word "Iberian" (from Iberus) indicates their presence in the peninsula of that name. The conclusion which I urge, therefore, is, that the correlatives of the art of the "new race" will be found in the "Oasis magna." That some of the tombs contain Egyptian and even Mediterranean relics is readily explained by the commerce which it is evident from the figures of their boats they soon established on the Nile.



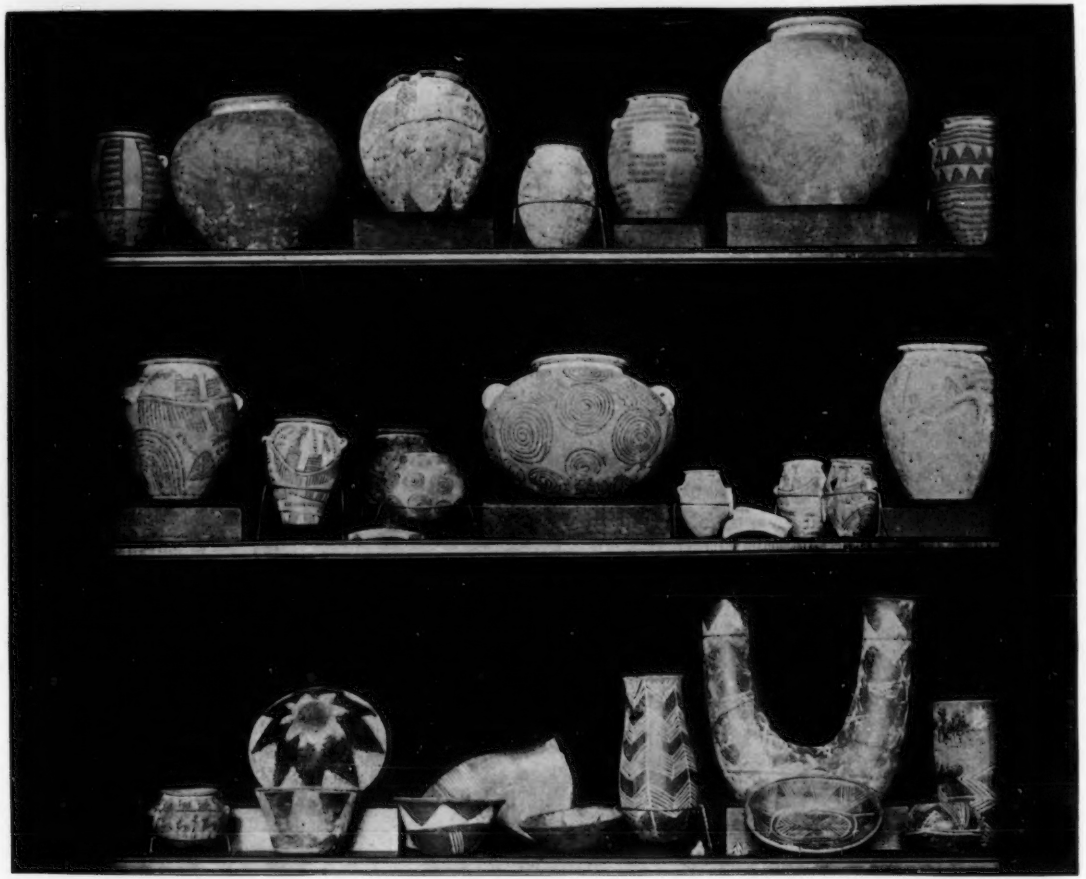
No. 1.

No. 2.

1.—STONE VASES TYPICALLY LIBYAN. 2.—BLACK AND BLACK AND RED POLISHED POTTERY DISTINCTLY LIBYAN.



COARSE POTTERY, JARS, COFFIN, LARGE BOWLS CONTAINING ASHES FROM THE FUNERAL FEAST.



IMPORTED DECORATED POTTERY FOUND ASSOCIATED WITH LIBYAN DEPOSITS.

Stated Meeting, March 20, 1896.

Vice-President, Dr. PEPPER, in the Chair.

Present, 22 members.

Correspondence was submitted as follows:

Letter of envoy from Mr. Robert N. Toppan, Cambridge, Mass.

Letters of acknowledgment from the Royal Society of New South Wales, Sydney (143–146); K. B. Astron.-meteorologische Observatorium, Triest, Austria (142–147); Prof. Dr. F. Müller, Vienna, Austria (147); Oberhessische Gesellschaft für Natur- und Heilkunde, Giessen, Germany (147); K. Sächs. Gesellschaft d. Wissenschaften, Leipzig (143, 146, 147); Marquis Antonio De Gregorio, Palermo, Italy (147); Prof. E. D. Cope (147, 148), Mr. F. Prime, Philadelphia (147); Prof. John F. Carll, Pleasantville, Pa. (148); Lieut. A. B. Wyckoff, N. Yakima, Washington (148).

Letters of acknowledgment (149) from the Laval University, Quebec, Canada; Canadian Institute, Toronto, Canada; Bowdoin College Library, Brunswick, Me.; N. H. Historical Society, Concord; Vermont Historical Society, Montpelier; Amherst College Library, Mass.; Mass. Historical Society, Boston Athenæum, Boston Society of Natural History, Dr. Samuel A. Green, Boston, Mass.; Museum of Comparative Zoölogy, Harvard College, Profs. W. W. Goodwin, F. W. Putnam, Mr. Robert N. Toppan, Cambridge, Mass.; Essex Institute, Salem, Mass.; Amer. Antiquarian Society, Worcester, Mass.; Agricultural Experiment Station, Kingston, R. I.; Providence Franklin Society, Brown University Library, Providence, R. I.; Mr. George F. Dunning, Farmington, Conn.; Conn. Historical Society, Hartford; Buffalo Library, Society of Natural Sciences, Buffalo, N. Y.; Prof. Edward North, Clinton, N. Y.; Profs. T. F. Crane, J. M. Hart, Ithaca, N. Y.; Astor Library, N. Y. Academy of Medicine, Columbia College, Historical Society, Amer. Museum of Natural History, N. Y. Hospital, Prof. Joel Asaph Allen, Hon.

Charles P. Daly, Mr. J. Douglas, Dr. Daniel Draper, New York, N. Y.; Prof. Robert W. Rogers, Madison, N. J.; Profs. W. Henry Green, Charles W. Shields, Princeton, N. J.; Dr. Robert H. Alison, Ardmore, Pa.; Prof. Thomas C. Porter, Easton, Pa.; Mr. John Fulton, Johnstown, Pa.; Linnæan Society, Lancaster, Pa.; Dr. James W. Robins, Merion, Pa.; Historical Society, Academy of Natural Sciences, Engineers' Club, Franklin Institute, Library Co. of Philadelphia, Pennsylvania Hospital, Wagner Free Institute, Numismatic and Antiquarian Society, Profs. John Ashhurst, E. D. Cope, F. A. Genth, Henry D. Gregory, Lewis M. Haupt, James MacAlister, Benjamin Sharp, Drs. W. G. A. Bonwill, John H. Brinton, Edward A. Foggo, Persifor Frazer, George H. Horn, Frank W. Lewis, Morris Longstreth, John Marshall, George R. Morehouse, Charles A. Oliver, William Pepper, Charles Schäffer, Charles Stewart Wurts, Messrs. R. Meade Bache, Henry C. Baird, Cadwalader Biddle, George Tucker Bispham, Joel Cook, Jacob B. Eckfeldt, Charles C. Harrison, William A. Ingham, Benjamin Smith Lyman, Franklin Platt, J. Sergeant Price, Theodore D. Rand, J. G. Rosengarten, Julius F. Sachse, Coleman Sellers, F. D. Stone, W. P. Tatham, Joseph Willcox, Philadelphia; Mr. Heber S. Thompson, Pottsville, Pa.; Rev. F. A. Muhlenberg, Reading, Pa.; Dr. W. H. Appleton, Swarthmore, Pa.; Mr. Thomas S. Blair, Tyrone, Pa.; Philosophical Society, Mr. Philip P. Sharples, West Chester, Pa.; Agricultural Experiment Station, Newark, Del.; Maryland Institute for the Promotion of the Mechanic Arts, Enoch Pratt Free Library, Prof. Ira Remsen, Baltimore, Md.; University of Virginia, Charlottesville; West Virginia University, Morgantown; Georgia Historical Society, Savannah; Athenæum, Columbia, Tenn.; Newberry Library, Chicago, Ill.

Accessions to the Library were reported from the Comité de Conservation des Monuments de l'Art Arabe, Cairo, Egypt; Koloniaal Museum, Haarlem, Holland; Bataafsch Genootschap der Proefondervindelijke Wijsbegeerte, Rotterdam, Holland; Magyar Tudományos Akademia, Budapest, Hun-

gary; Württembergische Kommission für Landesgeschichte, Stuttgart; *Il Nuovo Cimento*, Pisa, Italy; Bibliothèque de la Faculté des Sciences, Marseilles, France; Prof. Gabriel de Mortillet, St. Germain-en-Laye, France; Mr. Charles Sedelmeyer, Paris, France; R. Academia de Ciencias y Artes, Barcelona, Spain; R. Academia de Ciencias, etc., Madrid, Spain; R. Meteorological Society, London, England; Mr. Robert Noxon Toppan, Cambridge, Mass.; General Alumni Society of University of Pennsylvania, College of Physicians, Profs. E. D. Cope, Theophilus Parvin, Philadelphia; Bureau of Education, Washington, D. C.; Western Society of Engineers, American Humane Association, Prof. Edmund J. James, Chicago, Ill.; Bishop Crescencio Carrillo, Merida, Yucatan; Instituto Médico Nacional, Mexico, Mex.; Agricultural Experiment Stations, Lake City, Fla., Fayettesville, Ark., Manhattan, Kans., Corvallis, Oreg., St. Anthony Park, Minn.

Mr. J. G. Rosengarten read an obituary notice of Rev. W. H. Furness, D.D.

Mrs. Stevenson read a paper on the recent discovery in Egypt of non-Egyptian remains. Numerous specimens were exhibited, principally pottery, showing various shapes of development. These belonged to a race which had invaded Egypt 3500 or 2800 B.C., bringing its customs without adopting much from the country occupied by it.

Dr. Frazer moved the thanks of the Society to Mrs. Stevenson for her address. Adopted.

Dr. Brinton objected to the identification of the Libyans with the neolithic tribes. In his view they were near relatives of the tribes now known as Berbers. In his opinion the invaders descended on Abydos from the Oasis Magna.

Pending nomination 1346 and new nomination 1347 were read.

Dr. Greene offered a resolution of inquiry, why certain omissions were made in the records of the proceedings.

The Secretaries explained that the communication was informal and without motion and seemed to have no place in the minutes.

On the resolution being put to vote, the yeas and nays being called, it was lost by 12 nays to 5 ayes.

Dr. Brinton offered the following:

Resolved, That papers by non-members be read by title only, except when the author is present, or by unanimous consent of the Society.

The resolution was referred by consent of mover to Council.

The rough minutes were read, and the Society adjourned.

APRIL 3 being Good Friday and a legal holiday, the meeting was postponed, by direction of the President, until April 10.

Stated Meeting, April 10, 1896.

Vice-President, Dr. PEPPER, in the Chair.

Present, 18 members.

Correspondence was submitted as follows:

Acknowledgments of election to membership from M. G. Bertin, Paris, France, March 15, 1896; Mr. Henry A. Pilsbry, Philadelphia, March 30, 1896.

Circular letter from the Principal and Vice-Chancellor of the University of Glasgow and the Lord Provost of Glasgow, on behalf of the Committee of Arrangements of Jubilee of the Right Hon. Prof. Lord Kelvin, on the completion of the fiftieth year of his tenure of the Chair of Natural Philosophy in the University of Glasgow, requesting the Society to appoint a representative to take part in the celebration, June 15 and 16, 1896.

An invitation, on parchment, from the University of Princeton, N. J., to attend its one hundred and fiftieth anniversary.

On motion, these letters were referred to the President, with power to appoint representatives.

Letters of envoy from the K. K. Astronomisch-Meteorologische Observatorium, Triest, Austria; K. Leopoldinisch-Carolinische Deutsche Akademie der Naturforscher, Halle a. S., Prussia; R. Accademia di Scienze, Lettere ed Arti,

Modena, Italy; Ministero di Pubblica Istruzione, Rome, Italy; Mr. James Douglas, New York, N. Y.; Field Columbian Museum, Chicago, Ill.; Museo de la Plata, Provincia de Buenos Aires, S. A.

Letters of acknowledgment from the Vogtländische Altertumsforschende Verein, Hohenleuben, Saxony (143, 146, 147); I. R. Accademia degli Agiati, Rovereto, Austria (142–147); K. Leopoldinisch-Carolinische Deutsche Akademie der Naturforscher, Halle a. S., Prussia (146, 147, and *Trans.*, xviii, 2); Academy of Science, Rochester, N. Y. (148); Prof. Charles A. Young, Princeton, N. J. (148); Mr. L. A. Scott, Philadelphia (148).

Letters of acknowledgment (149) from the Geological Survey, Ottawa, Canada; Manitoba Historical and Scientific Society, Winnipeg; Public Library, State Library, Boston, Mass.; Prof. C. H. Hitchcock, Hanover, N. H.; Prof. James Hall, Albany, N. Y.; Editor of *Popular Science Monthly*, Profs. C. F. Chandler, Isaac H. Hall, J. J. Stevenson, New York, N. Y.; Vassar Brothers' Institute, Poughkeepsie, N. Y.; Academy of Science, Geological Society of America, Rochester, N. Y.; Oneida Historical Society, Utica, N. Y.; New Jersey Historical Society, Newark; Prof. Charles A. Young, Princeton, N. J.; Prof. Martin H. Boyè, Coopersburg, Pa.; American Academy of Medicine, Prof. J. W. Moore, Easton, Pa.; State Library of Pennsylvania, Harrisburg; Hon. James T. Mitchell, Rev. H. Clay Trumbull, Drs. C. N. Peirce, Wm. H. Wahl, Messrs. Samuel Dickson, Patterson Du Bois, Philip C. Garrett, L. A. Scott, Frank Thomson, C. Tower, Jr., Philadelphia; Lackawanna Institute of History and Science, Scranton, Pa.; Weather Bureau, U. S. Naval Observatory, U. S. Geological Survey, U. S. Patent Office, Coast and Geodetic Survey, War Department, Dr. W. J. Hoffman, Prof. Chas. A. Schott, Washington, D. C.; Mr. T. L. Patterson, Cumberland, Md.; Mr. Jedediah Hotchkiss, Staunton, Va.; Elisha Mitchell Scientific Society, Chapel Hill, N. C.; South Carolina College, Columbia; University of Alabama, University P. O.; University of California, Prof. Joseph Le Conte, Berkeley, Cal.; Lick

Observatory, Mt. Hamilton, Cal.; Historical Society, State Mining Bureau, San Francisco, Cal.; Prof. J. C. Branner, Stanford University, Cal.; Geological Survey of Missouri, Jefferson City; Oberlin College, Oberlin, O.; Cincinnati Observatory, Cincinnati, O.; Prof. J. L. Campbell, Crawfordsville, Ind.; Prof. G. W. Hough, Evanston, Ill.; University Library, Champaign, Ill.; Dr. M. D. Ewell, Chicago, Ill.; Academy of Natural Sciences, Davenport, Ia.; State Historical Society of Iowa, Iowa City; University of Wisconsin, State Historical Society, Madison, Wis.; Kansas University *Quarterly*, Lawrence; Academy of Science, Washburn College Library, Topeka, Kans.; Colorado Scientific Society, Denver; Agricultural Experiment Stations—Geneva, N. Y.; Auburn, Ala.; Michigan Agricultural College, Ingham Co.; Ames, Ia.; Lincoln, Neb.; Corvallis, Oreg.; Tucson, Ariz.

Accessions to the Library were reported from the Geological Survey of India, Calcutta; Linnean Society of New South Wales, Sydney; Soc. Finno Ougrienne, Helsingfors, Finland; Ministerie van Binnenlandsche Zadun, s' Gravenhage, Netherlands; Osservatorio Astron. Meteorol., Triest, Austria; Akad. der Wissenschaften, R. Friedländer und Sohn, Berlin, Prussia; K. Leopold.-Carol. Akademie, Halle a. S., Prussia; Bayer. Numismat. Gesellschaft, München; R. Ministero della Instruzione Publica, Padova, Italia; R. Accad. di Scienze, etc., Modena, Italia; École Nat'l Supt. des Mines, Mr. Georges Bertin, Paris, France; Geographical Society, Manchester, Eng.; Canadian Institute, Ontario Archæological Museum, Mr. J. M. Clark, Toronto, Canada; Mr. George M. Whitaker, Boston, Mass.; Academy of Sciences, Amer. Museum Nat. History, Mr. James Douglas, New York, N. Y.; Free Public Library, Jersey City, N. J.; Lafayette College, Easton, Pa.; Pepper Laboratory of Clinical Medicine, Dr. Charles A. Oliver, Messrs. Wharton Barker, Frederick Prime, Maxwell Sommerville, Philadelphia; Lighthouse Board, U. S. Department of Agriculture, U. S. Geological Survey, Prof. Albert S. Gatschet, Washington, D. C.; University of California, Berkeley; State Historical Society, Madison, Wis.; State Historical Society,

Iowa City, Ia.; Agricultural Experiment Stations—College Park, Pa.; Lexington, Ky.; Columbia, Mo.; Agricultural College, Michigan; Madison, Wis.; Denver, Colo.; Berkeley, Cal.; Instituto Medico Nacional, Laminas, Mexico.

Photograph for the Society's Album was received from Dr. W. G. A. Bonwill, Philadelphia.

The following death was announced: Hon. William Strong, Washington, D. C., August 19, 1895.

A paper was read on the "Identification of Colored Inks by the Absorption Spectra," by Dr. C. A. Doremus, of New York.

Prof. Cope made some remarks on the figures of men and animals on a tablet from Nippur, and expressed the opinion that the men were of the pure white race and not mixed.

Dr. Brinton followed, corroborating the views of Prof. Cope.

Pending nominations 1346 and 1347, and new nominations 1348 to 1362, were read. On motion, the nominations of non-residents were referred to Council.

The Curators reported on the collections of coins and medals formerly deposited with the Numismatic Society, but at present deposited in the Pennsylvania Museum and School of Industrial Art. All the articles had been accounted for with but two exceptions.

The report was received, and the Curators discharged from further consideration of the subject.

The rough minutes were then read, and the Society adjourned.

The Identification of Colored Inks by their Absorption Spectra.

By Charles A. Doremus.

From the committee appointed by the Society to investigate the various methods for the examination of documents.

(*Read before the American Philosophical Society, April 10, 1896.*)

The substitution of aniline dyes for other coloring matters in the preparation of colored inks, especially red, necessitates the adoption of means for their recognition.

A characteristic feature of the aniline colors is a surface iridescence, distinguishable even in the thinnest layers.

The beetle bronze is unmistakable. The iridescence is frequently complementary to that of the color—thus green to red.

Many of these inks also show fluorescence. This is especially developed in very dilute solutions. Highly attenuated solutions of fluorescein behave differently to light from concentrated ones. The dichroism of concentrated solutions is quite distinct from the fluorescence obtained by dilution.

Concentration appears to destroy fluorescence. This is also true of glass. Glass containing ten per cent. of uranium oxide would not be recognized as the uranium glass whose greenish yellow fluorescence is so well known.

The writer was led to investigate many of these properties in connection with a case tried in New Jersey in 1891.* The circumstances were briefly as follows: Mr. George P. Gordon, of printing press fame, left a large estate by a will dated 1873. This will was rejected because the subscribing witnesses would not swear to the execution of it. The case became one of intestacy and was taken in charge by the Public Administrator of Brooklyn. The estate was then settled with the parties named in the will. The widow and a daughter by a first wife were the chief beneficiaries. The daughter died in 1890 and her will was offered for probate in New York city. A contest took place. The contesting attorneys received a letter from a party stating that he had seen a notice of the contest in the daily press and that they would hear something to their advantage should they communicate with him. This led to the finding (?) in a garret of a will purporting to have been executed by George P. Gordon in 1868. The subscribing witnesses to this document were all dead. The wife and daughter had also died before this alleged will was brought to light. This document was proved *ex parte* in New Jersey and ancillary probate was allowed in New York. The instrument was also filed in Trenton. The legal representatives of the heirs of the wife and daughter contested the genuineness of this will. The proponents were parties contesting the daughter's will to whom was joined Henry C. Adams, who claimed to have drawn the will and who would be benefited should it be established. For a time the litigation was conducted on the part of the contestants in attempts to prove by the handwriting that the signature of the testator was a forgery. The case to this point rested entirely upon expert testimony, when Adams brought forward a draft of the will purporting to have been made in July, 1868, and offered it in evidence. This draft was interlineated and amended with red ink. When submitted to expert chemists they pronounced the ink one of some aniline color and from general appearances eosine. The controversy then centred on the

*The Prerogative Court of the State of New Jersey in the matter of the Probate in solemn Form and the Last Will and Testament of George P. Gordon, deceased. *Jersey City News Press, 1891.*

question as to whether the ink was eosine or not. Experts were called for both sides and the writer was among those retained by the executors. As the right to use reagents on the document was denied all the preliminary tests had to be of a physical character, though they were afterwards verified by chemical tests in court. My attention had been called several years previously to the black appearance of the lips of players using rouge, one kind of which I knew to be eosine. Eosine is irresponsive to yellow rays and seems almost black in the glare of the footlights. Carmine and other reds retain more of their red color. Experiments were therefore made with different red inks, as carmine, aniline red, safranine, and eosine, and their appearance noted under monochromatic illumination of a sodium flame.

The results were not as pronounced as desired. Recourse was then had to comparing the various inks in strong daylight behind differently colored glasses. The effects were very striking, especially with the aniline inks since they possess iridescence.

Colored glasses also greatly aid in the discovery of their fluorescent qualities.

The ink on the document presented a lustre when illuminated through green glass which was quite different from that of carmine and various aniline inks.

The *fluorescence* of eosine may also be enhanced by the use of blue or purple glass.

These experiments induced the writer to try a spectroscopic examination of inks, both in solution and in form of writing.

A Zeiss micro-spectroscopic eye-piece and low-power lens were used at first, then a higher power. This test is especially valuable since the document is uninjured.

It requires the brightest sunlight as a source of illumination. The ink is viewed by transmitted light and an absorption spectrum is obtained. When mapped the spectra are found to vary.

This means of identification was, however, not sufficiently developed to enable it to be used in court, nor could it be shown because of the absence of proper facilities.

At court the preliminary examination of the experts was strengthened by chemical tests applied to the ink on the document and prominently the action of hydrochloric acid which produced a yellow color and by the greenish yellow fluorescent nature of a solution of the ink.

The opinion of the experts for the defense that the ink was eosine was corroborated by several ink manufacturers and a well-known importer of aniline dyes.

In rebuttal it was claimed that the ink was aurine.

It was necessary to break the evidence going to prove the ink to be eosine since that color was not discovered until 1874, eight years after the date of the will. Aurine was, however, in commercial use in 1865, and

as per patent of Henry Ellis, Great Britain, No. 2267. It was not shown, however, that it was purchasable as ink in this country in 1868.

The decision of the Chancellor in favor of the contestants was sustained in 1894 by the Court of Errors and Appeals.

While an alkaline aurine solution produces an ink very similar to eosine in many properties and reactions, it differs widely in others and especially in not having greenish yellow fluorescence of eosine in diluted solution and in not showing the same absorption spectrum and derivative spectra.

The accompanying maps show the spectra observed with thin layers of various inks. A Donné lactoscope proved very useful in varying the thickness of the layer until the most characteristic appearance was obtained. The same absorption bands were afterwards recognized when pen marks made with these inks were examined under a microscope to which a Zeiss spectroscopic eye-piece was adjusted.

The spectroscopic examination of the ink while on the document should be followed whenever allowed by observations of the spectra produced when the ink is subjected to the action of chemicals.

Very marked changes occur, and since even colorless solutions may show absorption bands this means of identification possesses the double advantage of an accurate physical test without injury to the document together with a combined chemical and physical test where the application of reagents is permitted.

Stated Meeting, April 17, 1896.

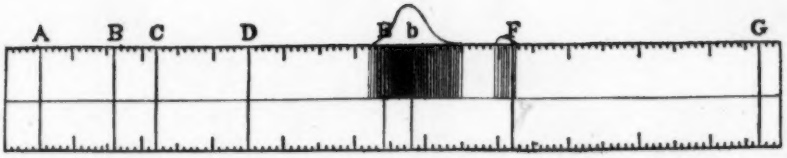
President, Mr. FRALEY, in the Chair.

Present, 20 members.

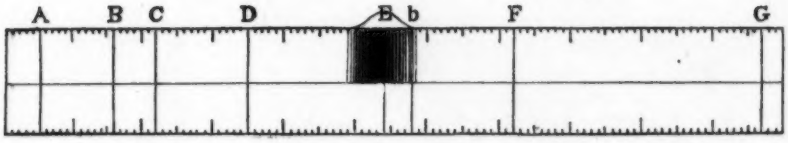
Mr. Georges Bertin, a newly elected member, was presented and took his seat.

Minutes of meeting of April 10 were read and approved.

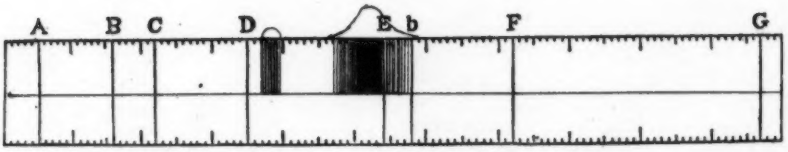
Letters of acknowledgment were received from the Public Library, Wellington, N. Z. (147); Universitatis Lundensis, Lund, Sweden (147); Profs. Friedrich Müller, Edward Suess, Vienna, Austria (148); Naturforschende Gesellschaft, Bamberg, Bavaria (147); K. Sächs. Meteorol. Institut, Chemnitz (148); Verein für Erdkunde, Dresden, Saxony (147, 148); Wetterauische Gesellschaft, Hanau, Germany (147); Verein für



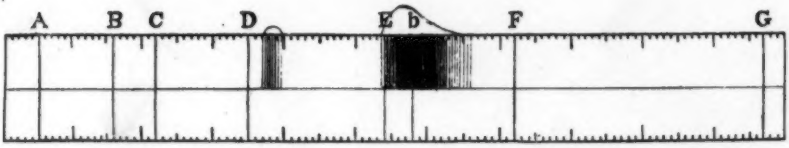
EOSINE IN WATER.



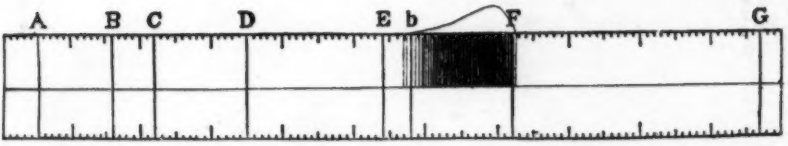
AURINE IN DILUTE ALKALI.



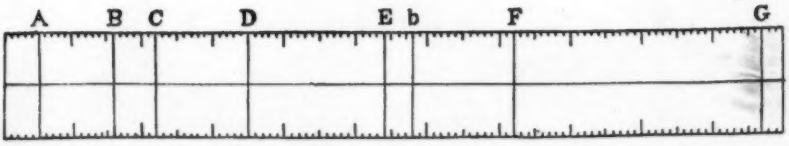
CARMINE IN AMMONIUM HYDRATE.



DAVID'S BRILLIANT CARMINE INK.



SAFRANINE.



Kunst und Alterthum, Ulm, Germany (143, 146, 147); R. Accademia di Scienze Lett. Arti, Modena, Italy (147); Texas Academy of Science, Austin (149); Kansas State Historical Society, Topeka (148); Observatorio Estado de Vera Cruz, Jalapa (144, 147, 149); Don Mariana Barcena, Observatorio Meteorologico, Mexico, Mex. (149).

Accessions to the Library were reported from the Genootschap van Kunsten en Wetenschappen, Batavia, Java; Nederlandsche Maatschappij ter bevordering, etc., Amsterdam, Netherlands; K. Universitetet, Lund, Sweden; Roemer Museum, Hildesheim, Prussia; Deutsche Geologische Gesellschaft, Berlin, Prussia; Académie des Sciences, Paris, France; Prof. Henry Wilde, Manchester, Eng.; Hon. J. M. LeMoine, Quebec, Canada; Amer. Antiquarian Society, Worcester, Mass.; Academy of Natural Sciences, Mr. A. E. Outerbridge, Philadelphia; U. S. Senate, U. S. Dep't of Agriculture, Washington, D. C.; California Academy of Sciences, San Francisco.

On behalf of the special committee in charge of the quarterly meetings, Dr. Pepper reported the details of that to occur May 1.

Dr. Brinton then read an obituary of the late Henry Hazlehurst.

Prof. Cope gave a brief account of his investigation of the remains found at Port Kennedy, the result so far being mammalia, 38; birds, 3; reptiles, 6.

In reply to Dr. Brinton, Prof Cope stated that the general term Plistocene is applied to the age of the deposit. It is part of Cenozoic times, beginning with a depression of probably 2200 feet, its middle corresponding with an elevation which had much to do with the preservation of the continental ice-cap. After this was a period of subsidence leaving but little land above the water.

In reply to Prof. Prime, Prof. Cope stated that Prof. Spencer had observed the depression along the New England coast.

Prof. Prime thought that no great depression could have

occurred in Pennsylvania, as the terminal moraine in Northampton county is but 650 feet above sea-level and is practically unchanged.

Pending nominations 1346 to 1362 were read.

The Library Committee reported through Dr. Greene and asked for an appropriation for the purchase of books and the filling of lacunæ.

On motion duly seconded the following was adopted:

Resolved, That an appropriation of $500 be made to the Committee on Library for expenses during the year 1896.

The rough minutes were read, and the Society adjourned.

Stated Meeting, May 1, 1896.

Vice-President, Dr. PEPPER, in the Chair.

Present, 39 members.

Correspondence was submitted.

The death of Jean Baptiste Léon Say, on April 21, was announced.

A letter was read by the Chairman from Dr. J. S. Minot, regretting his inability to be present and take part in the discussion.

A letter from the President announced that he had appointed Hon. Craig Biddle to represent this Society at the sesqui-centennial of Princeton University, and Dr. J. Cheston Morris at the semi-centennial jubilee of Lord Kelvin, at the University of Glasgow.

Prof. Cope was then called upon and opened the discussion of the "Factors of Organic Evolution," from the Palæontological standpoint.

Prof. Conklin followed, presenting the subject from an Embryological point of view.

Prof. L. D. Bailey, of Cornell, presented the subject from its Botanical aspect.

Dr. D. G. Brinton then presented his views of the subject.

The three original speakers were then called upon and each supplemented his remarks by thoughts suggested by the others.

In the course of his remarks, Prof. Cope exhibited two specimens illustrative of generalized types of Vertebrata. One of these was a cast of a species of the genus Phenacodus, from the Eocene, which represents the family from which all the Ungulate Placental Mammalia have descended. The other was a part of the skeleton of a reptile from the Permian, of the new genus Otocœlus. This genus is the type of a new family of the order Cotylosauria. This order approaches most nearly of all the Reptilia to the class Batrachia. It is also the most generalized of the Reptilia, and from it all other orders of the class have probably descended by modifications in different directions. The particular family Otocœlidæ differs from the other families of Cotylosauria in the possession of a meatus auditorius externus and of an osseous carapace. From it were probably descended the orders of Pseudosuchia and Testudinata, which first appear in the Trias. A description of this family and the species it includes will be given in an early number of the PROCEEDINGS of the Society.

Nominations 1346 to 1362 were read.

The Society was then adjourned.

The meeting of the first of May having been designated as that on which a discussion of the theme, "Factors of Organic Evolution," should be held, Prof. Cope, to whom the Special Committee in charge of the preparations for this meeting had confided the task of opening the discourse, presented an epitome of the subject as it exists to-day from the standpoint of paleontology.*

* [Prof. Cope, being unwilling to furnish the Society with the text of his remarks, or to have the stenographic copy printed in the PROCEEDINGS, his part of the joint discussion must be necessarily omitted.—SECRETARIES.]

Discussion of the Factors of Organic Evolution from the Embryological Standpoint.

By Prof. E. G. Conklin.

(*Read before the American Philosophical Society, May 1, 1896.*)

Up to the beginning of this decade embryology was largely dominated by the phylogeny idea. Individual development was generally studied, as the paleontologist studies his fossils, with a view to deciphering the evolutionary record in the various stages. It is now generally recognized, however, that embryology is but little fitted for the service into which it was so long forced, viz., the determining of phylogenies. The only safe guide in this matter is comparative anatomy of both living and extinct forms. On the other hand, our knowledge of the mechanics of evolution must always depend in large part upon the study of individual development. More than any other discipline, embryology holds the keys to the *method of evolution*. If ontogeny is not a true recapitulation it is, at least, a true *type* of evolution, and the study of the causes of development will go far to determine the factors of phylogeny.

The causes and methods of evolution are intimately bound up with those general phenomena of life such as assimilation, growth, differentiation, metabolism, inheritance, and variation; and the evolution problem can never be solved except through a study of these general phenomena of life itself. Our great need at present is not to know more of the course of evolution, but to discover, if possible, the causes of growth, differentiation, repetition, and variation. All these general phenomena are most beautifully illustrated in the development of individual organisms, and because they are fundamental to *any* theory of evolution I shall dwell upon them rather than upon the evidences for the Lamarckian or the Darwinian factors.

I call your attention very briefly to the following propositions: 1. Development, and consequently evolution, is the result of the interaction of extrinsic and intrinsic causes. 2. Intrinsic causes are dependent upon protoplasmic structure. 3. Inherited characters must be predetermined in the structure of the germinal protoplasm. 4. Germinal, as compared with somatic, protoplasm is relatively stable and continuous, but not absolutely so as maintained by Weismann; therefore, extrinsic causes may modify both germinal and somatic protoplasm. 5. It is extremely difficult to determine whether or not extrinsic factors have modified the structure of the germinal protoplasm. This is illustrated by some of the evidences advanced for the inherited effects of (1) diminished nutrition, (2) changes in environment, (3) use and disuse. 6. Experiment alone can furnish the crucial test of these Lamarckian factors.

1. The causes of development in general are usually recognized as twofold, extrinsic and intrinsic. As examples of extrinsic causes may be mentioned gravity, surface tension, light, heat, moisture, and chemism in general; examples of intrinsic causes are the non-exosmosis of salts from living bodies in water, the pouring of a glandular secretion or the sap of plants into a cavity under high pressure, the active changes in shape and position on the part of cells, assimilation, growth, division, etc. There is not, however, a uniformly sharp and distinct line of demarcation between these two factors of development. Phenomena once supposed to be due entirely to intrinsic causes are now known to be the result of extrinsic ones, and it is practically certain that this will be found true of still other phenomena. But although it is not possible to draw any hard and fast line between these two classes of causes, one can, in general, recognize a very marked difference between them. Extrinsic causes may, in large part, supply the stimulus and the energy for development, and may more or less modify its course; the intrinsic causes are of a much more complex character than the extrinsic ones, they are inherent in the living matter and in large part predetermine the course of development. In one form or another the distinction between these two classes of causes is recognized by all naturalists. His calls the intrinsic causes "the law of growth," the extrinsic ones the conditions under which that law operates. These designations correspond, at least in part, to Prof. Cope's Anagenesis and Katagenesis, and to Roux's "simple and complex components" of developmental processes.

While it is necessary to emphasize the differences between these two classes of causes, it is not intended thereby to dogmatically assert their total difference in kind. It may well be that these extrinsic and intrinsic causes are totally different in kind, but in our present state of ignorance it would be unjustifiable to affirm it. On the other hand, it would be just as unwarrantable to dogmatically affirm that there is no difference in kind between these two classes of causes, and that, therefore, all vital phenomena are only the manifestations of heat, light, electricity, attraction, repulsion, chemism, and the like. It may be that this is true, but there is as yet no sufficient evidence for it, and to attempt, as certain dynamical and mechanical hypotheses do, to refer all vital phenomena directly to such simple components as those named above is practically to make impossible at present any explanation of vital phenomena. "If we would advance without interruption," says Roux,* "we must be content, for many years to come, with an analysis into complex components."

2. We need not now further concern ourselves with an explanation of ***extrinsic*** causes or ***simple*** components, since this subject properly belongs to chemistry and physics. If, however, we examine more closely some of the ***intrinsic*** causes or ***complex*** components, we will find

*Wilhelm Roux, *Einleitung. Archiv für Entwicklungsmechanik der Organismen.*

that they are always associated with more or less complex *structures; in fact, that they are dependent upon structure.*

The smallest and simplest mass of protoplasm that can manifest all the fundamental phenomena of life, such as assimilation, growth, division, and metabolism, is an entire cell, nucleus and cytoplasm, and probably centrosome. The cell is composed, as microscopic study plainly reveals, of many dissimilar but perfectly coadapted parts, each performing its specific function, and it may therefore properly be called an *organism.* Some phenomena of cell life may be directly referred to the various visible constituents of the cell, but many of them are evidently connected with structures which we cannot see, structures which may perhaps never be seen, and yet which must be vastly more complex than the most complex molecules known to chemistry, and yet much more simple than the microsomes, centrosomes, and chromosomes which are visible in the cell. With these ultra-microscopical particles many of the most fundamental phenomena of life are associated, viz., assimilation, growth, metabolism, and probably differentiation, repetition, and variation. These functions are so coördinated that there can be no question that the ultra-microscopical structure is an *organization,* with part coadapted to part. The organization of the cell, therefore, does not stop with what the microscope reveals, but must be supposed to extend to the smallest ultimate particles of living matter which manifest specific functions. These are the vital units so generally postulated, the "smallest parts" of living matter, as they were called by Brücke, who first demonstrated that they must exist; the "physiological units" of Spencer, the "gemmules" of Darwin, the "micella-groups" of Nageli, the "pangenes" of De Vries, the "plasomes" of Wiesner, the "idioblasts" of Hertwig, the "biophores" of Weismann. Such ultimate units have been found absolutely necessary to explain those most fundamental of all vital phenomena, *assimilation* and *growth,* while many other phenomena, especially *particulate inheritance,* the *independent variability of parts,* and the hereditary transmission of *latent* and *patent characters,* can at present only be explained by referring them to ultra-microscopical units of structure. To deny that there are such units does not simplify the problem, as some seem to suppose, but renders it impossible of approach. A corpuscular hypothesis of life, like that of light, may be only a temporary makeshift, but it is better than nothing.

Whitman* well says: "Brücke's great merit consists in this that he taught us the necessity of assuming structure as the basis of vital phenomena, in spite of the negative testimony of our imperfect microscopes. That function presupposes structure is now an accepted axiom, and we need only extend Brücke's method of reasoning, from the tissue cell to the egg cell, in order to see that there is no escape from the

* C. O. Whitman, *The Inadequacy of the Cell-Theory of Development,* Biological Lectures, 1893.

conclusion that the whole course of developmental phenomena must be referred to organization of some sort. Development, no less than other vital phenomena, is a function of organization."

3. A study of the phenomena of development, as well as the principle of causality, make it certain that all the characters of the species are predetermined within the protoplasm of the fertilized egg cell. From a frog's egg only a frog will develop, from an echinoderm egg only an echinoderm, and the course of the development is, under normal circumstances, definitely marked out in each case, even down to the minutest details. All the results of experiment, as well as observation and induction, only serve to render this conclusion the more certain. It should be observed that to affirm that characters are predetermined is a very different thing from saying they are preformed. The one merely asserts that the cause of the transformations which lead from one step to another in the development is determined by the initial conditions of the fertilized egg cell; the other affirms that those transformations have already taken place.

The absolute determinism of development depends primarily upon the constant structure of the egg cell, but also to a certain extent upon a definite relation to extrinsic factors. Since, however, these extrinsic factors may be exactly the same in two cases, and yet the result of development be very different (*e. g.*, the egg of the starfish and that of the sea urchin), we can only conclude that while ontogenetic differences may be caused by a disturbance of the extrinsic factors, *inherited characters* are always the result of a definite structure of the germinal protoplasm, and that, therefore, development is, in the words of Prof. Whitman, "a function of organization."

Inheritance and variation are general terms which include a great many different kinds of phenomena, many of which seem to be due to entirely different factors. A great many phenomena of inheritance seem to be due entirely to extrinsic forces, but a more careful inquiry always reveals the fact that they are invariably due to the reaction of certain extrinsic causes on a perfectly definite living structure. As examples may be mentioned the following:

(1) The tiger-like striping of the egg of Fundulus, which is very characteristic and would certainly be regarded as an inherited character, has been shown by Loeb* to be due entirely to the position of the blood vessels of the blastoderm. The pigment cells are at first uniformly distributed, but when the blood vessels are formed they gather around them, probably through chemotropic action, and thus the characteristic banded appearance is produced. Graf has since shown that the color paterns of leaches are produced in the same way. It is not necessary, therefore, to assume that the color paterns in these cases are specifically represented in the germinal protoplasm; it may

*Jacques Loeb, *Some Facts and Principles of Physiological Morphology*, Biological Lectures, 1893.

even be that the position of the blood vessels is not so represented, but there must be some ultimate cause back in the germinal plasm itself which determines the series of causes which finally produces the color paterns. In short, this feature, like most others, was predetermined from the beginning.

(2) Herbst * has shown in a series of interesting experiments that by the use of various chemical substances the development of echinoderms may be profoundly modified. For example, in sea water deficient in calcium-chloride, or in which there is an excess of potassium-chloride, the pluteus larva, instead of developing calcareous spicules and the long ciliated arms which give the normal larva an angular, easel-shaped appearance, remains rounded in shape much like the larva of Balanoglossus, in which no spicular skeleton is developed. The withdrawal, therefore, of certain normally present substances from the environment may profoundly modify the end result. But in this case, as in the other, it is absolutely certain that the calcareous spicules were predetermined in the egg cell, although in the absence of calcareous matter from the water those spicules could not be built—the plan was there, but the building material was lacking.

Such modifications resulting from unusual conditions of pressure, temperature, density, nutrition—in fact, any alteration of the chemical or physical environment—may appear in any stage of development from the unsegmented egg to the adult condition, but it must not be supposed that the entire development can be reduced to such factors. Loeb argues that we do not inherit our body heat from our parents because it depends upon certain chemical processes, but is it not absolutely certain that we inherit a certain protoplasmic structure which determines those chemical processes, and hence the body temperature? To assume that extrinsic causes determine whether there shall hatch from an egg a chicken or an eagle is the sheerest nonsense. The study of extrinsic factors in relation to inheritance will serve to simplify some of the intricate problems to be explained, but surely no one believes that development can ever be referred entirely to such factors. The fact is that determinism, which is the most fundamental characteristic of inheritance, is manifested at every step of development, and there is certainly no escape from the conclusion that this determinism depends upon protoplasmic structure, and that this structure it is which is transmitted from generation to generation and which forms the physical basis of inheritance.

All really inherited characters must, therefore, be represented in the structure of the germinal protoplasm, and must consequently be present from the beginning of development. "We must consider it as a law derivable from the causality principle," says Hatschek,† "that in

Zeit. wiss. Zool., Bd. lv.

† Berthold Hatschek, *Ueber die Entwicklungsgeschichte von Teredo*, Arb. Zool. Inst., Wien, 1880.

the phylogenetic alterations of an animal form the end stages are not alone altered, but the entire series from the egg cell to the end stage. Every alteration of an end stage or addition of a new one must be caused by an alteration of the egg cell itself." Nägeli * has expressed a similar view in the following famous sentence: "Egg cells must contain all the essential characteristics of the species as perfectly as do adult organisms, and hence they must differ from one another, no less as egg cells than in the fully developed state. The species is contained in the egg of the hen as completely as in the hen, and the hen's egg differs as much from the frog's egg as the hen from the frog."

4. The remarkable tenacity of inheritance, as shown especially in reversions and the preservation of useless and embryonic characters through many hundreds or thousands of generations, and amid the most diverse circumstances, bears strong testimony to the great stability of that living structure which is the basis of inheritance. On the other hand, all experience goes to prove that the living substance of the body cells in general is readily modified, and that in a surprisingly short time. The fact of this great difference cannot fail to be recognized; its cause is at present merely a matter of conjecture.

Weismann at one time supposed the cause of this to be an absolutely stable, absolutely separate, and perpetually continuous germ plasm. However, there is the most convincing and abundant evidence that although the germ plasm is relatively very stable and continuous, it does not possess those divinely perfect characters ascribed to it. More recently Weismann has expressly abandoned each and all of these characters,† and now, like a good Lamarckian, finds "the cause of hereditary variation in the direct effects of external influences on the biophores and determinants."

The outcome of the whole matter, then, is that we find ourselves much in the same position as we were before Weismann denied the possibility of the inheritance of acquired characters. *All hereditary variations are caused by the action of extrinsic forces on the germinal protoplasm, producing changes in its structure.* Strangely enough, this proposition was admitted as a logical necessity by one who undertook by rigorous logic to prove the reverse. Since almost the only objection to this position was the one raised by Weismann, it may now be considered as definitely settled, and the only question before us, then, is: How can extrinsic causes modify the structure of the germinal protoplasm?

Since by his own admissions, as Romanes has shown, the most characteristic features of Weismann's system, both as to inheritance and evolution, have been virtually abandoned, it seems to some that his theories have been of no real value, and that, like an *ignis fatuus*, they have only served to lead biologists astray far from the path of science into the dangerous quagmires of speculation. I do not share any such

* Nägeli, *Mechanisch-physiologische Theorie der Abstammungslehre*, 1884.

† See Romanes' *Examination of Weismannism*, 1893.

opinion. Apart from his splendid observations and the great stimulus to investigation which Weismann's theories have furnished, there remain many elements of permanent value in his work.

Osborn * thinks that Weismann's most "permanent service to biology is his demand for direct evidence of the Lamarckian principle." It seems to me that his greatest service consists in the emphasis which he has laid upon the intrinsic factors of development and evolution as opposed to the extrinsic factors, a thing which he has indeed overemphasized, but which has sadly needed a strong defender in these later years. Largely as an outcome of his work, we now recognize the possibilities and the limitations of the selection theory as never before, and we also recognize that many of the evidences which were adduced in support of the Lamarckian factors are not conclusive, while the method of securing conclusive evidence is clearly marked out. Whatever we may think of his theories, this certainly is no slight service.

5. It is by no means an easy task to determine whether the influence of extrinsic forces has really reached the germinal protoplasm and modified its structure; much more difficult is it to determine how that modification takes place. I believe it is safe to say that a majority of the cases which are supposed to prove the inheritance of acquired characters prove only that characters are acquired, not that they are inherited. There is great need of caution against supposing that any character is inherited unless it repeats itself under many and different conditions. Apart altogether from inheritance, similar conditions may produce similar results, and consequently this source of error must be eliminated if we would be certain that the structure of the germinal protoplasm has really been modified. Many of the alleged cases of the inheritance of mutilations, or the direct influence of the environment and of use and disuse fall away under this precaution.

The general evidence for the inheritance of mutilations is so notoriously bad that I pass it by altogether, and select for consideration a few cases, chosen from a recent work on the subject,† which have by various writers been alleged as showing the direct influence of environment in modifying species and also the inherited effects of use and disuse.

(1) It is well known that certain gasteropods, if reared in small vessels, are smaller than when grown in large ones, and this case has been cited as showing the influence of environment in modifying species. There is good evidence, however, that this modification does not affect the germinal protoplasm, for these same gasteropods will grow larger if placed in larger vessels. It seems very probable that the diminished size of these animals is due to deficient food supply, but this has so little modified the somatic protoplasm that, although they may be fully developed as shown by sexual maturity, they at once

* Osborn, *The Unknown Factors of Evolution*, Biological Lectures, 1894.

† E. D. Cope, *The Primary Factors of Organic Evolution*, 1896.

increase in size as soon as more abundant food is provided, and this takes place by the active growth and division of all the cells of the body. In higher animals, once maturity has been reached, there is little chance for growth, apparently because many of the cells are so highly differentiated that they can no longer divide. Consequently the growth is limited, and hence the size of the adult may depend in part upon the amount of nutriment furnished to the embryo. This limitation of growth is due to the high degree of differentiation of the somatic cells. But as the germ cells are not highly differentiated and are capable of division, it follows that they would not be permanently modified by starving. It may be, as Prof. Brewer argues, that long continued starving and consequent dwarfing of animals may leave its mark on the germinal plasm; but, as he also remarks, this influence must be very slight as compared with the cumulative effects of selection in breeding, and it is safe to assert that there is no such wholesale and immediate modification of the germinal plasm due to the influence of nutrition as some people seem to suppose.

(2) The interesting experiments of Schmankewitsch in transforming one species of Artemia into another by gradually increasing the salinity of the water, or in transforming Artemia into another genus, Branchinecta, by decreasing the salinity of the water are well known, and are often cited as illustrations of the fact that specific and even generic differences may suddenly be produced under the influence of the environment. The very fact, however, that these changes are suddenly produced, and that they can at will be quickly modified in one direction or the other is evidence that they are not represented in the structure of the germinal plasm, and the fact that definite extrinsic causes, such as salt or fresh water, acting upon this plasm produces results which are constantly the same is the best evidence that the internal mechanism, *i. e.*, the structure of the germinal plasm, is constantly the same. The same can be said of many artificially produced modifications, such as the exogastrulas and potassium larvæ of Herbst, all of which profound changes are due entirely to extrinsic and not to intrinsic causes, as is shown by the fact that they disappear as soon as the immediate extrinsic cause is withdrawn. The same thing is shown in Poulton's experiments on the colors of Lepidopterous larvæ, and in this case also it is known that the changes are not inherited, at least during the limited period through which the experiments were conducted; and it should be observed that to assume that this would take place at the end of an indefinite number of generations is simply to beg the question.

Very many other cases of a similar character might be instanced under this head if time permitted, but I hasten on to another class of evidence.

Under the subject of the inherited effects of use and disuse the following cases may be mentioned as showing how inconclusive much of this evidence is:

(1) In the first place, this whole line of argument starts with the assumption that the individual habits of an animal are inherited, and that these habits ultimately determine the structure—an assumption which really begs the whole question; for, after all, the substratum of any habit must be some physical structure, and if modified habits are inherited it must be because some modified structure is inherited. I take an example which will serve as an illustration of a whole class: Jackson* says that the elongated siphon of Mya, the long-necked clam, is due to its habit of burrowing in the mud, or to quote his words: "It seems very evident that the long siphon of this genus was brought about by the effort to reach the surface, induced by the habit of deep burial." It certainly would be pertinent to inquire where it got this habit, and how it happened to be transmitted. It is surely as difficult to explain the acquisition and inheritance of habits, the basis of which we do not know, as it is to explain the acquisition and inheritance of structures which are tangible and visible. Such a method of procedure, in addition to begging the whole question, commits the further sin of reasoning from the relatively unknown to the relatively known!

This case is but a fair sample of a whole class, among which may be mentioned the following: The derivation of the long hind legs of jumping animals, the long fore legs of climbing animals, and the elongation of all the legs of running animals through the influence of an inherited habit. All such cases are open to the very serious objection mentioned above.

(2) Another whole class of arguments may be reduced to this proposition: Because necessary mechanical conditions are never violated by organisms, therefore modifications due to such conditions show the inheritance of acquired characters. Plainly, the alternative proposition is this: If acquired characters are not inherited, organisms ought to do impossible things.

(3) Many of the arguments advanced to prove the inheritance of characters acquired through use or disuse seem to me to prove entirely too much. For example, Prof. Cope argues very ably that bones are lengthened by both stretch and impact, and that modifications thus produced are inherited. Even granting that this is true, how would it be possible for this process of lengthening to cease, since in active animals the stretch and impact must be continual? Prof. Cope answers that the growth ceases when "equilibrium" is reached. I confess I cannot understand this explanation, since the assumed stimulus to growth must be continual. But granting again that growth may stop when an animal's legs become long enough to "satisfy its needs," how on this principle are we to account for the *shortening* of legs, as, for example, in the turnspit dog and the ancon sheep and numberless cases occurring in nature? If any one species was able, by taking thought of mechanical stresses and strains, to add one cubit unto its stature, how could the same stresses and strains be invoked to decrease its stature?

* R. T. Jackson, *Memoirs Boston Soc. Nat. Hist.*, 1890.

These evidences are, I know, not the strongest ones which can be adduced in support of the Lamarckian factors. There are at present a relatively small number of such arguments which seem to be valid and the great force of which I fully admit. But the cases which I have cited are, I believe, fair samples of the majority of the evidences so far presented, and in the face of such "evidence" it is not surprising that one who is himself a profound student of the subject and a convinced Lamarckian prays that the Lamarckian theory may be delivered from its friends.*

6. Another line of evidence, and by far the most promising, is that of direct experiment. So far most of the experiments which have been carried on to determine this question have been carried only half way to a conclusion—they have shown that characters are acquired, they have usually failed to show that they are transmitted to descendants. Among animals one of the best known cases is the inheritance of epilepsy and other disorders in Guinea pigs, due to certain nervous lesions of the parents. But Romanes,† who spent much time in trying to corroborate these results, concludes as follows: "On the whole, then, as regards Brown-Sequard's experiments, it will be seen that I have not been able to furnish any approach to a full corroboration."

Among plants, on the other hand, there is more and better experimental evidence, but it is not by any means as full or satisfactory as could be wished. Of one thing we may be certain: a satisfactory solution of the problem can be reached only by experiment. The mere observations and inductions of the morphologist, while affording valuable collateral evidence, can never furnish the crucial test. As long as we deal merely with probabilities of a low order there will be profound differences of opinion: *e. g.*, Cope believes in all the Lamarckian factors; Romanes rejects use and disuse, but believes in the others; Weismann rejects all of them. Why? Is it because each does not know the facts upon which the others build? Certainly not. Those so-called facts are merely probabilities of a higher or lower order, and to one man they seem more important than to another. No conviction based even upon a high degree of probability can ever be reached in this way. There is here a deadlock of opinion, each challenging the other to produce indubitable proof. This can never be furnished by observation alone. Possibly even experiment may fail in it, but at least it is the only hope.

Conclusion.

On the whole, then, I believe the facts which are at present at our disposal justify a return to the position of Darwin. Neither Weismannism nor Lamarckism alone can explain the causes of evolution. But Darwinism can explain those causes. Darwin endeavored to show that

* H. F. Osborn, *Evolution and Heredity*, Biological Lectures, 1890.

†G. J. Romanes, *Post-Darwinian Questions*, 1895.

variations, perhaps even adaptations, were the result of extrinsic factors acting upon the organism, and that these variations or adaptations were increased and improved by natural selection. This is, I believe, the only ground which is at present tenable, and it is but another testimony to the greatness of that man of men, that, after exploring for a score of years all the ins and outs of pure selection and pure adaptation, men are now coming back to the position outlined and unswervingly maintained by him.

Finally, we ought not to suppose that we have already reached a satisfactory solution of the evolution problem, or are, indeed, near such a solution. "We must not conceal from ourselves the fact," says Roux, "that the causal investigation of organism is one of the most difficult, if not the most difficult, problem which the human intellect has attempted to solve, and that this investigation, like every causal science, can never reach completeness, since every new cause ascertained only gives rise to fresh questions concerning the cause of this cause."

The Factors of Organic Evolution from a Botanical Standpoint.

By Prof. L. H. Bailey.

(*Read before the American Philosophical Society, May 1, 1896.*)

THE SURVIVAL OF THE UNLIKE.

We all agree that there has been and is evolution; but we probably all disagree as to the exact agencies and forces which have been and are responsible for it. The subject of the agencies and vehicles of evolution has been gone over repeatedly and carefully for the animal creation, but there is comparatively little similar research and speculation for the plant creation. This deficiency upon the plant side is my excuse for calling your attention, in a popular way, to a few suggestions respecting the continuing creation of the vegetable world, and to a somewhat discursive consideration of a number of illustrations of the methods of advance of plant types.

1. *Nature of the Divergence of the Plant and Animal.*

It is self-evident that the development of life upon our planet has taken place along two divergent lines. These lines originated at a common point. This common life-plasma was probably at first more animal-like than plant-like. The stage in which this life-plasma first began to assume plant-like functions is closely and possibly exactly preserved to us in that great class of organisms which are known as mycetozoa when studied by zoölogists and as myxomycetes when studied by botanists. At one stage

of their existence, these organisms are amœba-like, that is, animal-like, but at another stage they are sporiferous or plant like. The initial divergencies in organisms were no doubt concerned chiefly in the methods of appropriating food, the animal-like organisms apprehending their food at a more or less definite point, and the plant-like organisms absorbing food throughout the greater or even the entire part of their periphery. It is not my purpose to trace the particular steps or methods of these divergencies, but to call your attention to what I believe to be a fundamental distinction between the two lines of development, and one which I do not remember to have seen stated in the exact form in which it lies in my mind.

Both lines probably started out with a more or less well-marked circular arrangement of the parts or organs. This was consequent upon the peripheral arrangement of the new cells in the development of the multicellular organism from the unicellular one. A long line of animal life developed in obedience to this peripheral or rotate type of organization, ending in the echinoderms and some of the mollusks. This line long ago reached its zenith. No line of descent can be traced from them, according to Cope. The progressive and regnant type of animal life appeared in the vermes or true worms, forms which are characterized by a two-sided or bilateral, and therefore more or less longitudinal, structure. The animal-like organisms were strongly developed in the power of locomotion, and it is easy to see that the rotate or centrifugal construction would place the organism at a comparative disadvantage, because its seat of sensation is farthest removed from the external stimuli. But the worm-like organisms, "being longitudinal and bilateral," writes Cope, "one extremity becomes differentiated by first contact with the environment." In other words, the animal type has shown a cephalic, or head-forming, evolution in consequence of the bilateralism of structure. The individual has become concentrated. Out of this worm-form type, therefore, all the higher ranges of zoötypic evolution have sprung, and one is almost tempted to read a literal truth into David's lamentation that "I am a worm and no man."

If, now, we turn to plants we find the rotate or peripheral arrangement of parts emphasized in all the higher ranges of forms. The most marked bilateralism in the plant world is amongst the bacteria, desmids, and the like, in which locomotion is markedly developed; and these are also amongst the lowest plant types. But plants soon became attached to the earth, or, as Cope terms them, they are "earth parasites." They therefore found it to their advantage to reach out in every direction from their support in the search for food. Whilst the centrifugal arrangement has strongly tended to disappear in the animal creation, it has tended with equal strength to persist and to augment itself in the plant creation. Its marked development amongst plants began with the acquirement of terrestrial life, and with the consequent evolution of the asexual or sporophytic type of vegetation. Normally, the higher type of plant bears its

parts more or less equally upon all sides, and the limit to growth is still determined by the immediate environment of the given individual or of its recent ancestors. Its evolution has been acephalic, diffuse, or headless, and the individual plant or tree has no proper concentration of parts. For the most part, it is filled with unspecialized plasma, which, when removed from the parent individual (as in cuttings and grafts), is able to reproduce another like individual. The arrangements of leaves, branches, the parts of the flower, and even of seeds in the fruit, are thus rotate or circular, and in the highest type of plants the annual lateral increments of growth are disposed in like fashion; and it is significant to observe that in the compositæ, which is considered to be the latest and highest general type of plant-form, the rotate or centrifugal arrangement is most emphatically developed. The circular arrangement of parts is the typical one for higher plants, and any departure from this form is a specialization and demands explanation.

The point I wish to urge, therefore, is the nature of the obvious or external divergence of plant-like and animal-like lines of ascent. The significance of the bilateral structure of animal-types is well understood, but this significance has been drawn, so far as I know, from a comparison of bilateral or dimeric animals with rotate or polymeric animals. I want to put a larger meaning into it, by making bilateralism the symbol of the onward march of animal evolution and circumlateralism (if I may invent the term) the symbol of plant evolution. The suggestion, however, applies simply to the general arrangement of the parts or organs of the plant body, and has no relation whatever to functional attributes or processes. It is a suggestion of analogues, not of homologues. We may, therefore, contrast these two great lines of ascent, which, with so many vicissitudes, have come up through the ages, as Dipleurogenesis and Centrogenesis.

The two divergent directions of the lines or phyla of evolution have often been the subject of comment, but one of the sharpest contrasts between the two was made in 1884 by Cope, when he proposed that the vegetable kingdom has undergone a degenerate or retrogressive evolution. "The plants in general," he then wrote, "in the persons of their protist ancestors, soon left a free-swimming life and became sessile. Their lives thus became parasitic, more automatic, and, in one sense, degenerate." The evolution of the plant creation is, therefore, held to be a phenomenon of catagenesis or decadence. This, of course, is merely a method of stating a comparison with the evolution of the animal line or phylum, and is therefore of the greatest service. For myself, however, I dislike the terms retrogressive, catagenetic, and the like, as applied to the plant creation, because they imply intrinsic or actual degeneracy. True retrogressive or degenerate evolution is the result of loss of attributes. Cope holds that the chief proof of degeneracy in the plant world is the loss of a free-swimming habit, but it is possible that the first life-plasma was stationary; at any rate, we do not know that it was motile. Degen-

eracy is unequivocally seen in certain restricted groups where the loss of characters can be traced directly to adaptive changes, as in the loss of limbs in the serpents. Retarded evolution expresses the development of the plant world better than the above terms, but even this is erroneous because plant types exhibit quite as complete an adaptation to an enormous variety of conditions as animals do, and there has been rapid progress towards specialization of structure. As a matter of fact, the vegetable world does not exhibit, as a whole, any backward step, any loss of characters once gained, nor any stationary or retarded periods; but its progress has been widely unlike that of the animal world and it has not reached the heights which that line of ascent has attained. The plant phylum cannot be said to be catagenetic, but suigenetic. Or, in other words, it is centrogenetic as distinguished from dipleurogenetic.

The hearer should be reminded, at this point, of the curious alternation of generations which has come about in the plant world. One generation develops sexual functions, and the product of the sexual union is an asexual generation, and this, in turn, gives rise to another sexual generation like the first. In the lowest sex-plants, as the algæ, the sexual generation—or the gametophyte, as it is called—generally comprises the entire plant body, and the asexual generation—or sporophyte—develops as a part of the fructifying structure of the gametophyte, and is recognizable as a separate structure only by students of special training. In the fungi, which are probably of catagenetic evolution, alternation of generations is very imperfect or wanting. In the true mosses, the gametophyte is still the conspicuous part of the plant structure. It comprises all that part of the moss which the casual observer recognizes as "the plant." The sporophytic generation is still attached to the persistent gametophyte, and it is the capsule with its stem and appendages. In the ferns, however, the gametophytic stage is of short duration. It is the inconspicuous prothallus, which follows the germination of the spore. Therefrom originates "the fern," all of which is sporophytic, and the gametophyte perishes. With the evolution of the flowering plants, the gametophyte becomes still more rudimentary, whilst the sporophyte is the plant, tree or bush, as we see it. The gametophytic generation is associated with the act of fertilization, the male prothallus or gametophyte developing from the pollen grain and soon perishing, and the female prothallus or gametophyte developing in the ovule and either soon perishing or persisting in the form of the albumen of the seed. The great development of the sporophyte in later time is no doubt a consequence of the necessity of assuming a terrestrial life; and with this development has come the perfection of the centrogenic form.

2. *The Origin of Differences.*

The causes which have contributed to the origin of the differences which we see in the organic creation have been and still are the subjects of the

most violent controversy. Those persons who conceive these differences to have come into existence full-formed, as they exist at the present time, are those who believe in the dogma of special creations, and they usually add to the doctrine a belief in design in nature. This doctrine of special creation receives its strongest support when persons contrast individual objects in nature. Certainly nothing can seem more unlike in very fundamental character than an insect and an elephant, a star-fish and a potato, a man and an oak tree. The moment one comes to study the genealogies of these subjects or groups, however, he comes upon the astonishing fact that the ancestors are more and more alike the farther back they are traced. In other words, there are great series of convergent histories. Every naturalist, therefore, is compelled to admit that differences in nature have somehow been augmented in the long processes of time. It is unnecessary, therefore, that he seek the causes of present differences until he shall have determined the causes of the smallest or original differences. It is thus seen that there are two great and coördinate problems in the study of evolution, the causes of initial differences, and the means by which differences are augmented. These two problems are no doubt very often expressions of the same force or power, for the augmentation of a difference comes about by the origination of new degrees of difference, that is, by new differences. It is very probable that the original genesis of the differences is often due to the operation of the very same physiological processes which gradually enlarge the difference into a gulf of wide separation.

In approaching this question of the origin of unlikenesses, the inquirer must first divest himself of the effects of all previous teaching and thinking. We have reason to assume that all beings came from one original life-plasma, and we must assume that this plasma had the power of perpetuating its physiological identity. Most persons still further assume that this plasma must have been endowed with the property of reproducing all its characters of form and habit exactly, but such assumption is wholly gratuitous and is born of the age-long habit of thinking that like produces like. We really have no right to assume either that this plasma was or was not constituted with the power of exact reproduction of all its attributes, unless the behavior of its ascendants forces us to the one or the other conclusion. Inasmuch as no two individual organisms ever are or ever have been exactly alike, so far as we can determine, it seems to me to be the logical necessity to assume that like never did and never can produce like. The closer we are able to approach to plasmodial and unspecialized forms of life in our studies of organisms, the more are we impressed with the weakness of the hereditary power. Every tyro in the study of protoplasm knows that the amœba has no form. The shapes which it assumes are individual, and do not pass to the descendants. To my mind, therefore, it is a more violent assumption to suppose that this first unspecialized plasma should exactly reproduce all its minor features than to suppose that it had no distinct hereditary power and therefore, by

the very nature of its constitution, could not exactly reproduce itself. The burden of proof has been thrown upon those who attempt to explain the initial origin of differences, but it should really be thrown upon those who assume that life-matter was originally so constructed as to rigidly recast itself into one mould in each succeeding generation. I see less reason for dogmatically assuming that like produces like than I do for supposing that unlike produces unlike.

I advanced this proposition a year ago in my *Plant-Breeding* (pp, 9, 10), and I am now glad to find, since writing the above paragraph, that H. S. Williams has reached similar conclusions in his new *Geological Biology*. He regards mutability as the fundamental law of organisms, and speaks of the prevalent notion that organisms must necessarily reproduce themselves exactly as "one of the chief inconsistencies in the prevalent conception of the nature of organisms." "While the doctrine of mutability of species has generally taken the place of immutability," he writes, "the proposition that like produces like in organic generation is still generally, and I suppose almost universally, accepted. It therefore becomes necessary to suppose that variation is exceptional, and that some reason for the accumulation of variation is necessary to account for the great divergencies seen in different species. . . . . The search has been for some cause of the variation : it is more probable that mutability is the normal law of organic action, and that permanency is the acquired law." I do not suppose that Professor Williams makes definite variation an inherent or necessary quality of organic matter, but that this matter had no original hereditary power and that its form and other attributes in succeeding generations have been moulded into the environment, and that the burden of proof is thrown upon those who assume that life-matter was endowed with the property that like necessarily produces like. At all events, this last is my own conception of the modification of the streams of ascent.

In other words, I look upon heredity as an acquired character, the same as form or color or sensation is, and not as an original endowment of matter. The hereditary power did not originate until for some reason it was necessary for a given character to reproduce itself, and the longer any form or character was perpetuated, the stronger became the hereditary power.

It is now pertinent to inquire what determined the particular differences which we know to have persisted. The mere statement that some forms became sessile or attached to the earth, and that others became or remained motile, is an assumption that these differences were direct adaptations to environment. Every little change in environment incited a corresponding change in the plastic organization ; and the greater and more various the changes in the physical attributes of the earth with the lapse of time, the greater became the modifications in organisms. I believe, therefore, that the greater part of present differences in organisms are the result directly and indirectly of external stimuli, until we come

into those higher ranges of being in which sensation and volition have developed, and in which the effects of use and disuse and of psychological states have become increasingly more important as factors of ascent. The whole moot question, then, as to whether variations are definite or multifarious, is aside from the issue. They are as definite as the changes in the environment, which determine and control their existence. More differences arise than can persist, but this does not prove that those which are lost are any the less due to the impinging stimuli. Those who write of definite variation, usually construe the result or outcome of some particular evolution into a measure of the variation which is conceived to have taken place in the group. Most or all of the present characters of any group are definite because they are the survivals in a process of elimination; but there may have been, at various times, the most diverse and diffuse variations in the very group which is now marked by definite attributes. As the lines of ascent developed, and generation followed generation in countless number, the organization was more and more impressed with the features of ancestral characters, and these ancestral characters are the more persistent as they have been more constant in the past. But these characters, which appear as hereditary or atavistic variations in succeeding generations, were no doubt first, at least in the plant creation, the offspring, for the most part, of the environment reacting upon the organism. As life has ascended in the time-scale and has become increasingly complex, so the operation of any incident force must ever produce more diverse and unpredictable results. What I mean to say is that, in plants, some of the variations seem to me to be the resultants of a long line of previous incident impressions, or have no immediate inciting cause. Such variation is, to all appearances, fortuitous. It is, therefore, evident that the study of the effects of impinging environments at the present day may not directly elucidate the changes which similar conditions may have produced in the beginning.

Whilst the steadily ascending line of the plant creation was fitting itself into the changing moods of the external world, it was at the same time developing an internal power. Plants were constantly growing larger and stronger or more specialized. The accumulation of vital energy is an acquired character the same as peculiarities of form or structure are. It is the accumulated result of every circumstance which has contributed to the well-being and virility of the organism. The gardener knows that he can cause the plant to store up energy in the seed, so that the resulting crop will be the larger. Growth is itself but the expression or result of this energy which has been picked up by the way through countless ages. Now, mere growth is variation. It results in differences. Plants cannot grow without being unlike. The more luxuriant the growth, the more marked the variation. Most plants have acquired or inherited more growth-force than they are able to use because they are held down to certain limitations by the conditions in which they are necessarily placed by the struggle for existence. I am convinced that many of

the members of plants are simply outgrowths resulting from this growth-pressure, or as Bower significantly speaks of them ("A Theory of the Strobilus in Archegoniate Plants," *Annals of Botany*, viii, 358, 359), the result of an "eruptive process." The pushing out of shoots from any part of the plant body, upon occasion, the normal production of adventitious plantlets upon the stems and leaves of some begonias (especially *Begonia phyllomaniaca*), bryophyllum, some ferns, and many other plants, are all expressions of the growth-force which is a more or less constant internal power. This growth-force may give rise to more definite variations than impinging stimuli do; but the growth-force runs in definite directions because it, in its turn, is the survival in a general process of elimination. Many of the characters of plants which—for lack of better explanation—we are in the habit of calling adaptive, are no doubt simply the result of eruption of tissue. Very likely some of the compounding of leaves, the pushing out of some kinds of prickles, the duplication of floral organs, and the like, are examples of this kind of variation. We know that the characters of the external bark or cortex upon old tree trunks are the result of the internal pressure in stretching and splitting it. This simply shows how the growth-force may originate characters of taxonomic significance when it is expressed as mere mechanical power acting upon tissue of given anatomical structure. This power of growth is competent, I think, to originate many and important variations in plants. I suppose my conception of it to be essentially the same as that of the bathmism of Cope, and the "Theory of the Organic Growth" of Eimer.

We have now considered two general types of forces or agencies which start off variations in plants—purely external stimuli, and the internal acquired energy of growth. There is still a third general factor, crossing, or, as Eimer writes it, "sexual mixing." The very reason for the existence of sex, as we now understand it, is to originate differences by means of the union of two parents into one offspring. This sexual mixing cannot be considered to be an original cause of unlikenesses, however, since sex itself was at first a variation induced by environment or other agencies, and its present perfection, in higher organisms, is the result of the process of continuous survival in a conflict of differences.

The recent rise of Lamarckian views seems to have been largely the result of an attempt to discover the *vera causa* of variations. Darwin's hypothesis of natural selection assumes variability without inquiring into its cause, and writers have therefore said that Darwin did not attempt to account for the cause of variations. Nothing can be farther from his views. Yet some of our most recent American writings upon organic evolution repeat these statements. Cope, in his always admirable *Primary Factors of Organic Evolution*, writes that "Darwin only discussed variation after it came into being." Yet Darwin's very first chapter in his *Origin of Species* contains a discussion of the "Causes of Variability," and the same subject is gone over in detail in *Variation of Animals and Plants Under*

Domestication. Darwin repeatedly refers the cause or origin of variation to "changed conditions of life," which is essentially the position maintained by the Lamarckians, and he as strenuously combats those who hold that definite variation is an innate attribute of life. "But we must, I think, conclude . . . ." writes Darwin in the latter book, "that organic beings, when subjected during several generations to any change whatever in their conditions, tend to vary." He discussed, at length, the particular agencies which he considered to be most potent in inducing variability, and enumerated, amongst other factors, the kind and amount of food, climate and crossing. "Changes of any kind in the conditions of life," he repeats, "even extremely slight changes, often suffice to cause variability. Excess of nutriment is perhaps the most efficient single exciting cause." Cope, in his discussion of the "Causes of Variation," starts out with the proposition "to cite examples of the direct modifying effect of external influences on the characters of individual animals and plants;" and he closes with this paragraph: "I trust that I have adduced evidence to show that the stimuli of chemical and physical forces, and also molar motion or use and its absence, are abundantly sufficient to produce variations of all kinds in organic beings. The variations may be in color, proportions, or details of structure, according to the conditions which are present." This is, in great part, the thesis to which Darwin extended the proofs of a most laborious collection of data from gardeners and stock-breeders and from feral nature. It has been the great misfortune of the interpretation of Darwin's writings that his hypothesis of natural selection has so completely overtopped everything else in the reader's mind that other important matters have been overlooked.

Whilst the one central truth in the plant creation is the fact that differences arise as a result of variations in environment, there are nevertheless many exceptions to it. There are various types of differences which are merely incidental or secondary to the main stem of adaptive ascent. Some of these are such as arise from the cessation of the constructive agencies, and others are mere correlatives or accompaniment of type differences. As an example of the former, we may cite the behavior of the potato. By high cultivation and careful breeding, the plant has been developed to produce enormous crops of very large tubers, so heavy a crop that the plant has been obliged to spare some of its energy from the production of pollen and berries for the purpose of maintaining the subterranean product. It is evident that this high state of amelioration can be maintained only by means of high cultivation. The moment there is a let-down in the factors which have bred and maintained the plant, there is a tendency towards a breaking up and disappearance of the high-bred type. This is an illustration of the phenomenon of panmixia, as outlined by Weismann, except that the force which has ceased to act is human selection rather than natural selection. "This suspension of the preserving influence of natural selection," Weismann writes, "may be termed Panmixia." In

his opinion, "the greater number of those variations which are usually attributed to the direct influence of external conditions of life, are to be attributed to panmixia. For example, the great variability of most domesticated animals and plants essentially depends upon this principle." In other words, certain differences are preserved through the agency of natural selection, and certain differences are lost; if the organism is removed from this restraining and directing agency, all variations have the chance of asserting themselves. "All individuals can reproduce themselves," Weismann explains, "and thus stamp their characters upon the species, and not only those which are in all respects, or in respect to some single organ, the fittest." I am convinced that this term expresses a very important truth, and one which, as Weismann says, is particularly apparent in domestic animals and plants; but panmixia does not express an incident force. If new differences arise in consequence of the cessation of the directive agency of natural selection, it is because they were first impressed upon the organization by some unaccountable agency; or, if there is simply a falling away from accumulated characters, the residuary or secondary features which appear are probably the compound and often deteriorated result of various previous incident forces. In short, panmixia is a name for a class of phenomena, and it cannot be considered as itself an original cause of variation. It is, to my mind, largely the unrestrained expression or unfolding of the growth-force consequent upon the removal of the customary pressure under which the plant has lived.

3. *The Survival of the Unlike.*

The one note of the modern evolution speculations which has resounded to the remotest corner of civilization, and which is the chief exponent of current speculation respecting the origin and destiny of the organic world, is Spencer's phrase, "the survival of the fittest." This epigram is an epitome of Darwin's law of natural selection, or "the preservation, during the battle for life, of varieties which possess any advantage in structure, constitution or instinct." In most writings, these two phrases —"natural selection" and "the survival of the fittest"—are used synonymously; but in their etymology they really stand to each other in the relation of process and result. The operation of natural selection results in the survival of the fittest. One must not be too exact, however, in the literal application of such summary expressions as these. Their particular mission is to afford a convenient and abbreviated formula for the designation of important principles, for use in common writing and speech, and not to express a literal truth. Darwin was himself well aware of the danger of the literal interpretation of the epigram "natural selection." "The term 'natural selection,'" he writes, "is in some respects a bad one, as it seems to imply conscious choice; but this will be disregarded after a little familiarity." This technical use of the term "natural selection" is now generally accepted unconsciously; and yet there have been recent revolts against it upon the score that it does not

itself express a literal principle or truth. If we accept the term in the sense in which it was propounded by its author, we are equally bound to accept "survival of the fittest" as a synonymous expression because its author so designed it. "By natural selection or survival of the fittest," writes Spencer, "by the preservation in successive generations of those whose moving equilibria happen to be least at variance with the requirements, there is eventually produced a changed equilibrium completely in harmony with the requirements."

It should be said that there is no reason other than usage why the phrase "survival of the fittest" should not apply to the result of Lamarckian or functional evolution as well as of Darwinian or selective evolution. It simply expresses a fact without designating the cause or the process. Cope has written a book upon the "Origin of the Fittest," in which the argument is Lamarckian. The phrase implies a conflict, and the loss of certain contestants and the salvation of certain others. It asserts that the contestants or characters which survive are the fittest, but it does not explain whether they are fit because endowed with greater strength, greater prolificness, completer harmony with surroundings, or other attributes. I should like to suggest, therefore, that the chiefest merit of the survivors is unlikeness, and to call your attention for a few minutes to the significance of the phrase—which I have used in my teaching during the last year—the survival of the unlike.

This phrase—the survival of the unlike—expresses no new truth, but I hope that it may present the old truth of vicarious or non-designed evolution in a new light. It defines the fittest to be the unlike. You will recall that in this paper I have dwelt upon the origin and progress of differences rather than of definite or positive characters. I am so fully convinced that, in the plant creation, a new character is useful to the species because it is unlike its kin, that the study of difference between individuals has come to be, for me, the one absorbing and controlling thought in the contemplation of the progress of life. These differences arise as a result of every impinging force—soil, weather, climate, food, training, conflict with fellows, the strain and stress of wind and wave and insect visitors—as a complex resultant of many antecedent external forces, the effects of crossing, and also as the result of the accumulated force of mere growth; they are indefinite, non-designed, an expression of all the various influences to which the passive vegetable organism is or has been exposed; those differences which are most unlike their fellows or their parents find the places of least conflict, and persist because they thrive best and thereby impress themselves best upon their offspring. Thereby there is a constant tendency for new and divergent lines to strike off, and these lines, as they become accented, develop into what we, for convenience sake, have called species. There are, therefore, as many species as there are unlike conditions in physical and environmental nature, and in proportion as the conditions are unlike and local are the species well defined. But to nature, perfect adaptation is the end; she knows nothing, *per se*,

as species or as fixed types. Species were created by John Ray, not by the Lord ; they were named by Linnæus, not by Adam.

I must now hasten to anticipate an objection to my phrase which may arise in your minds. I have said that when characters are unlike existing characters they stand a chance of persisting ; but I do not desire to say that they are useful in proportion as they are unlike their kin. I want to express my conviction that mere sports are rarely useful. These are no doubt the result of very unusual or complex stimuli, or of unwonted refrangibility of the energy of growth, and not having been induced by conditions which act uniformly over a course of time, they are likely to be transient. I fully accept Cope's remark that there is "no ground for believing that sports have any considerable influence on the course of evolution. . . . . The method of evolution has apparently been one of successional increment and decrement of parts along definite lines." Amongst domestic animals and plants the selection and breeding of sports, or very unusual and marked variations, has been a leading cause of their strange and diverse evolution. In fact, it is in this particular thing that the work of the breeder and the gardener are most unlike the work of nature. But in feral conditions, the sport may be likened to an attribute out of place ; and I imagine that its chief effect upon the phylogeny of a race—if any effect it have—is in giving rise in its turn to a brood of less erratic unlikenesses. This question of sports has its psychological significance, for if the way becomes dark the wanderer invokes the aid of this *ignus fatuus* to cut short his difficulties. Sir William Thompson supposes that life may first have come to earth by way of some meteor, and Brinton proposes that man is a sport from some of the lower creation. It is certainly a strange type of mind which ascribes a self-centred and self-sufficient power to the tree of life, and then, at the very critical points, adopts a wholly extraneous force and one which is plainly but a survival of the old cataclysmic type of mind ; and it is the stranger, too, because such type of explanation is not suggested by observation or experiment, but simply by what is for the time an insuperable barrier of ignorance of natural processes. If evolution is true at all, there is reason to suppose that it extends from beginning to finish of creation, and the stopping of the process at obscure intervals is only a temporary satisfaction to a mind that is not yet fully committed to the eternal truth of ascent. The tree of life has no doubt grown steadily and gradually, and the same forces, variously modified by the changing physical conditions of the earth, have run on with slow but mighty energy until the present time. Any radical change in the plan would have defeated it, and any mere accidental circumstance is too trivial to be considered as a modifying influence of the great onward movement of creation, particularly when it assumes to account for the appearing of the very capstone of the whole mighty structure.

Bear with me if I recite a few specific examples of the survival of the unlike, or of the importance, to organic types, of gradually widening dif-

ferences. Illustrations might be drawn from every field of the organic creation, but I choose a few from plants because these are the most neglected and I am most familiar with them. These are given to illustrate how important external stimuli are in originating variation, and how it is that some of these variations persist.

Let me begin by saying that a good gardener loves his plants. Now, a good gardener is one who grows good plants, and good plants are very unlike poor plants. They are unlike because the gardener's love for them has made them so. The plants were all alike in November; in January, the good gardener's plants are strong and clean, with large dense leaves, a thick stem, and an abundance of perfect flowers; the poor gardener's plants are small and mean, with curled leaves, a thin hard stem, and a few imperfect flowers. You will not believe now that the two lots were all from the same seed-pod three months ago. The good gardener likes to save his own seeds or make his own cuttings; and next year his plants will be still more unlike his neighbor's. The neighbor tries this seed and that, reads this bulletin and that, but all avails nothing simply because he does not grow good plants. He does not care for them tenderly, as a fond mother cares for a child. The good gardener knows that the temperature of the water and the air, the currents in the atmosphere, the texture of the soil, and all the little amenities and comforts which plants so much enjoy, are just the factors which make his plants successful; and a good crop of anything, whether wheat or beans or apples, is simply a variation.

And do these unlikenesses survive? Yes, verily! The greater part of the amelioration of cultivated plants has come about in just this way,—by gradual modifications in the conditions in which they are grown, by means of which unlikenesses arise; and then by the selection of seeds from the most coveted plants. Even at the present day, there is comparatively little plant-breeding. The cultivated flora has come up with man, and if it has departed immensely from its wild prototypes, so has man. The greater part of all this has been unconscious and unintended on man's part, but it is none the less real.

As an illustration of how large the factors of undesigned choice and selection are in the amelioration of the domestic flora, let me ask your attention to the battle of the seed-bags. In the year 1890, the census records show, for the first time, the number of acres in the United States devoted to the growing of seed. I give the acreage of three representative crops, and these figures I have multiplied by the average seed-yields per acre in order to arrive at an approximate estimate of the entire crop produced, and the number of acres which the crop would plant. I have used low averages of yields in order to be on the safe side, and I have likewise used liberal averages of the quantity of seed required to plant an acre when making up the last column:

	Acres.	Average yield per acre.	Approximate crop.	Would plant.
Cabbage,	1,268	200 lbs.	253,600 lbs.	1,014,400 acres.
Cucumber,	10,219	120 "	1,226,280 "	613,140 "
Tomato,	4,356	80 "	368,480 "	1,473,920 "

The last column in this table has particular interest because it shows the enormous acreage which these seeds, if all planted, would cover. We are now curious to know if such areas really are planted to these species, and if they are not, it will be pertinent to inquire what becomes of the seeds. Unfortunately, we have no statistics of the entire acreages of these various truck-garden crops, but the same census gives the statistics of the commercial market gardens of the country. Inquiry of seed-merchants has convinced me that about one-fourth of all the seeds sold in any year go to market gardeners. I have therefore multiplied the census figures of market gardens by four for the purpose of arriving at an estimate of the total acreage of the given crops in the United States; and I have introduced the last column from the above table for purposes of comparison:

	Acreage of market gardens.	Probable total acreage.	There are seeds enough to plant	Difference.
Cabbage,	77,094	308,376	1,014,400 acres.	706,024 acres.
Cucumber,	4,721	18,884	613,140 "	594,256 "
Tomato,	22,802	91,208	1,473,920 "	1,382,712 "

It will thus be seen that there are enough cabbage seeds raised in this country each year—if the census year is a fair sample—to plant nearly three-quarters of a million acres more than actually are planted; about the same surplus of cucumber seeds; and a surplus of tomato seeds sufficient to plant over one and a quarter million acres. It is possible, of course, that the figures of actual acreage of these crops are too low; but such error, if it occur, must be much overbalanced by the large quantities of home-grown and imported seeds which are used every year. These startling figures would not apply so well to many other crops which are detailed in the census bulletin. For instance, the peas raised in this country would plant only about 46,000 acres, whilst there are over 100,000 acres actually grown; but this discrepancy is probably accounted for by the fact that the larger part of the seed peas are grown in Canada and therefore do not figure in our census. There is a somewhat similar discrepancy in the watermelon, but in this crop the seeds are very largely home-saved by the heavy planters in the South and West. I do not give these figures for their value as statistics, but simply for the purpose of graphically expressing the fact that many more seeds are raised by cultivators each average year than are ever grown into plants, and that the struggle for existence does not necessarily cease when plants are taken under the care of man.

What, now, becomes of this enormous surplus of seeds? Let us take a rough survey of the entire seed crop of any year. In the first place, a certain percentage of the seeds is laid aside by the seedsman as a surety against failure in the year to come. Much of this old stock never finds its way into the market and is finally discarded. We will estimate this element of waste as twenty per cent. Of the eighty per cent. which is actually sold, perhaps another ten per cent. is never planted, leaving about seventy per cent. which finds its way into the ground. These two items of loss are pure waste and have no effect upon the resulting crop. Now, of the seeds which are planted, not more than seventy-five per cent. can be expected to germinate. That is, there is certainly an average loss of twenty-five per cent. in nearly all seeds—and much more in some—due to inherent weakness, and seventy-five per cent. represents the survival in a conflict of strength. We have now accounted for about half of the total seed product of any year. The remaining half produces plants; but here the most important part of the conflict begins. In the crops mentioned above, much less than half of the seeds which are grown ever appear in the form of a crop. We must remember, moreover, that in making the estimate of the number of acres which these seeds would plant, I have used the customary estimates of the quantity of seeds required to plant an acre. Now, these estimates of seedsmen and planters are always very liberal. Every farmer sows from five to twenty times more seeds than he needs. Some years ago, I sowed seeds according to the recommendation of one of our best seedsmen, and I found that peas would be obliged to stand four-fifths of an inch apart, beets about twenty to the foot, and other vegetables in like confusion. I suppose that of all the seeds which actually come up, not more than one in ten or a dozen, in garden vegetables, ever give mature plants. What becomes of the remainder? They are thinned out for the good of those which are left.

This simple process of thinning out vegetables has had a most powerful effect upon the evolution of our domestic flora. It is a process of undesigned selection. This selection proceeds upon the differences in the seedlings. The weak individuals are disposed of, and those which are strongest and most unlike the general run are preserved. It is a clear case of the survival of the unlike. The laborer who weeds and thins your lettuce bed unconsciously blocks out his ideas in the plants which he leaves. But all this is a struggle of Jew against Jew, not of Jew against Philistine. It is a conflict within the species, not of species against species. It therefore tends to destroy the solidarity of the specific type, and helps to introduce much of that promiscuous unlikeness which is the distinguishing characteristic of domestic plants.

Let us now transfer this emphatic example to wild nature. There we shall find the same prodigal production of seeds. In the place of the gardener undesignedly moulding the lines of divergence, we find the inexorable physical circumstances into which the plastic organisms must grow, if they grow at all. These circumstances are very often the direct

causes of the unlikenesses of plants, for plants which start like when they germinate may be very unlike when they die. Given time and constantly but slowly changing conditions, and the vegetable creation is fashioned into the unlikenesses which we now behold. With this conception, let us read again Francis Parkman's picturesque description of the forest of Maine in his *Half-Century of Conflict:* "For untold ages Maine had been one unbroken forest, and it was so still. Only along the rocky seaboard, or on the lower waters of one or two great rivers a few rough settlements had gnawed slight indentations into this wilderness of woods, and a little farther inland some dismal clearing around a block-house or stockade let in the sunlight to a soil that had lain in shadow time out of mind. This waste of savage vegetation survives, in some part, to this day, with the same prodigality of vital force, the same struggle for existence and mutual havoc that mark all organized beings, from men to mushrooms. Young seedlings in millions spring every summer from the black mould, rich with the decay of those that had preceded them, crowding, choking and killing each other, perishing by their very abundance; all but a scattered few, stronger than the rest, or more fortunate in position, which survive by blighting those about them. They in turn, as they grow, interlock their boughs, and repeat in a season or two the same process of mutual suffocation. The forest is full of lean saplings dead or dying with vainly stretching towards the light. Not one infant tree in a thousand lives to maturity; yet these survivors form an innumerable host, pressed together in struggling confusion, squeezed out of symmetry and robbed of normal development, as men are said to be in the level sameness of democratic society. Seen from above, their mingled tops spread in a sea of verdure basking in light; seen from below, all is shadow, through which spots of timid sunshine steal down among legions of dark, mossy trunks, toadstools and rank ferns, protruding roots, matted bushes, and rotting carcases of fallen trees. A generation ago one might find here and there the rugged trunk of some great pine lifting its verdant spire above the indistinguished myriads of the forest. The woods of Maine had their aristocracy; but the axe of the woodman has laid them low, and these lords of the wilderness are seen no more."

In such bold and generalized examples as this, the student is able to discern only the general fact of progressive divergency and general adaptation to conditions, without being able to discover the particular directive forces which have been at the bottom of the evolution. It is only when one considers a specific example that he can arrive at any just conclusions respecting initial causes of modification. Of adaptive modifications, two general classes have been responsible for the ascent of the vegetable kingdom, one a mere moulding or shaping into the passive physical environments, the other the direct result of stress or strain imposed upon the organism by wind and water and by the necessities of a radical change of habit from aquatic to terrestrial life, and later on by the stimuli of insects upon the flowers. One of the very best examples of the mere pas-

sive ascent is afforded by the evolution of the root as a feeding organ; and a like example of development as a result of strain is afforded by the evolution of the stem and vascular or fibrous system. Our present flora, like our present fauna, is an evolution from aquatic life. The first sessile or stationary plants were undoubtedly stemless. As the waters increased in depth and plants were driven farther and farther from their starting points by the struggle for place and the disseminating influence of winds and waves, the plant body became more and more elongated. Whilst the plant undoubtedly still absorbed food throughout its entire periphery, it nevertheless began to differentiate into organs. The area chiefly concerned in food-gathering became broadened into a thallus, a constricted or stem-like portion tended to develop below, and the entire structure anchored itself to the rock by a hold-fast or grapple. This hold-fast or so-called root of most of our present sea-weeds is chiefly a means of holding the plant in place, and it probably absorbs very little food. As plants emerged into amphibian life, however, the foliar portion was less and less thrown into contact with food, and there was more and more demand upon the grapple which was anchored in the soil. The foliage gradually developed into organs for absorbing gases and the root was forced to absorb the liquids which the plant needed. I do not mean to say that there is any genetic connection between the sea-weeds and the higher plants, or that the roots of the two are homologous; but to simply state the fact that, in point of time, the hold-fast root developed before the feeding root did, and that this change was plainly one of adaptation. Specialized forms of flowering plants, which inhabit water, still show a root system which is little more than an anchor, and the foliage actively absorbs water. The same environmental circumstances are thus seen to have developed organs of similar physiological character in widely remote times and in diverse lines of the plant evolution. "As the soil slowly became thicker and thicker," writes King in his book upon *The Soil*, "as its water-holding power increased, as the soluble plant food became more abundant, and as the winds and the rains covered at times with soil portions of the purely superficial and aërial early plants, the days of sunshine between passing showers, and the weeks of drought intervening between periods of rain, became the occasions for utilizing the moisture which the soil had held back from the sea. These conditions, coupled with the universal tendency of life to make the most of its surroundings, appear to have induced the evolution of absorbing elongations, which, by slow degrees and centuries of repetition, came to be the true roots of plants as we now know them." Some aquatic flowering plants are, as we have seen, still practically rootless and they absorb the greater part of their food directly by the foliar parts; but the larger number of the higher plants absorb their mineral food by means of what has come to be a subterranean feeding organ, and the foliar parts have developed into gas-absorbing organs and they take in water only when forced to do so under stress of circumstances.

But as a mere feeding organ, the root requires no fibrous structure. It is still a hold-fast or grapple and its mechanical tissue has developed enormously, along with that of the stem, in order to preserve the plant against the strain of the moving elements and to maintain its erectness in aërial life. When this self-poised epoch arrives, the vegetable world begins its definite and steady ascent in centrogenic form. Whilst the animal creation leaves its centrogenic arrangement early in its own time-scale, the plant creation assumes such arrangement at a comparatively late epoch in its time-scale.

Perhaps the best illustration which I can bring you of the origin of the unlike by means of environmental conditions and the survival of some of this unlikeness in the battle for life, is the development of the winter quiescence of plants. What means all this bursting verdure of the liquid April days? Why this annually returning miracle of the sudden expansion of the leaf and flower from the lifeless twigs? Were plants always so? Were they designed to pass so much of their existence in the quiescent and passive condition? No. The first plants had no well-defined cycles, and they were born to live, not to die. There were probably no alternations of seasons in the primordial world. Day alternated with night, but month succeeded month in almost unbroken sameness age after age. As late as the Carboniferous time, according to Dana, the globe "was nowhere colder than the modern temperate zone, or below a mean temperature of 60° F." The earth had become wonderfully diverse by the close of the Cretaceous time, and the cycads and their kin retreated from the poles. Plants grew the year round; and as physical conditions became diverse and the conflict of existence increased, the older and the weaker died. So a limit to duration, that is, death, became impressed upon the individuals of the creation; for death, as seen by the evolutionist, is not an original property of life-matter, but is an acquired character, a result of the survival of the fittest. The earth was perhaps ages old, even after life began, before it ever saw a natural death; but without death all things must finally have come to a standstill. When it became possible to sweep away the old types, opportunity was left for new ones; and so the ascent must continue so long as physical conditions, which are not absolutely prohibitive of life, shall become unlike.

Species have acquired different degrees of longevity, the same as they have acquired different sizes and shapes and habits—by adaptation to their conditions of life. Annual plants comprise about half of the vegetable kingdom, and these are probably all specializations of comparatively late time. Probably the greater part of them were originally adaptations to shortening periods of growth, that is, to seasonal changes. The gardener, by forceful cultivation and by transferring plants towards the poles, is able to make annuals of perennials. Now, a true annual is a plant which normally ripens its seeds and dies before the coming of frost. Many of our garden plants are annuals only because they are killed by frost. They naturally have a longer season than our climate will admit, and some of

them are true perennials in their native homes. These plants are, with us, plur-annuals, and amongst them are the tomato, red pepper, egg plant, potato, castor bean, cotton, Lima bean and many others. But there are some varieties of potatoes and other plants which have now developed into true annuals, normally completing their entire growth before the approach of frost. It is all the result of adaptation to climate, and essentially the same phenomenon is the development of the annual and biennal flora of the earth from the perennial. An interesting example of the effect of climate upon the seasonal duration of plants is the indeterminate or prolonged growth of plants in England as compared with the same plants in America. The cooler summer and very gradual approach of winter in England develop a late and indefinite maturity of the season's growth. When English plants are grown in America, they usually grow until killed by fall frosts; but after a few generations of plants, they acquire the quick and decisive habit of ripening which is so characteristic of our vegetation. I once made an extended test of onions from English and American seeds (Bull. 31, Mich. Agric. College), and was astonished to find that nearly all of the English varieties continued to grow until frost and failed "to bottom," whilst our domestic varieties ripened up in advance of freezing weather. This was true even of the Yellow Danvers and Red Wethersfield, varieties of American origin and which could not have been grown very many years in England. Every horticulturist of much experience must have noticed similar unmistakable influences of climate upon the duration of plants.

A most interesting type of examples of the quick influence of climate upon plants—not only upon their duration but upon habit and structural characters—is that associated with the growing of "stock seed" by seedsmen. Because of uncertainties of weather in the Eastern States, it is now the practice to grow seeds of onions, Lima beans and other plants in California or other warm regions; but the plants so readily acquire the habit of long-continuing growth as to be thereafter grown with difficulty in the Northeastern States. It is, therefore, necessary that the seedsman shall raise his stock seed every year in his own geographical region, and this seed is each year sent to California for the growing of the commercial seed crop. In other words, the seed of California-grown onions is sold only for the purpose of growing onion bulbs for market, and is not planted for the raising of a successive crop of seed. This results in growing only a single generation of the crop in the warm country. Onion seed from stock which has been grown in California for several years produces onions which do not "bottom" well, much as I found to be the case with the English onion seed.

But many plants, in geologic time, could not thus shorten up their life-history to adjust themselves to the oncoming of the seasons. They ceased their labors with the approach of the cold or the dry, tucked up their tender tissues in buds and resigned themselves to the elements. If a man could have stood amongst those giant mosses and fern forests of

the reeking Carboniferous time, and could have known of the refrigeration which the earth was to undergo, he would have exclaimed that all living things must utterly perish. Consider the effects of a frost in May. See its widespread devastation. Yet, six months hence the very same trees which are now so blackened, will defy any degree of cold. And then, to make good the loss of time, these plants start into activity relatively much earlier in spring than the same species do in frostless climates. This very day, when frosts are not yet passed, our own New York hillsides are greener with surface vegetation than the lands of the Gulf States are, which have been frostless for two months and more. The frogs and turtles, the insects, the bears and foxes, all adjust themselves to a climate which seems to be absolutely prohibitive of life, and some animals may actually freeze during their hibernation, and yet these April days see them again in heyday of life and spirits! What a wonderful transformation is all this! This enforced period of quiescence is so impressed upon the organization that the habit becomes hereditary in plants, and the gardener says that his begonias and geraniums and callas must have a "rest," or they will not thrive. But in time he can so far break this habit in most plants as to force them into activity for the entire year. These budding days of April, therefore, are the songs of release from the bondage of winter which has come on as the earth has grown aged and cold.

I must bring still one more illustration of the survival of the unlike, out of the abundance of examples which might be cited. It is the fact that, as a rule, new types are variable and old types are inflexible. The student of fossil plants will recall the fact that the liriodendrons, ginkgos, sequoias, sassafrasses and other types came into existence with many species and are now going out of existence with one or two species. Williams has considered this feature, for extinct animal forms, at some length in his new *Geological Biology*. "Many species," he writes, "which by their abundance and good preservation in fossil state give us sufficient evidence in the case, exhibit greater plasticity in their characters at the early stage than in later stages of their history. A minute tracing of lines of succession of species shows greater plasticity at the beginning of the series than later, and this is expressed, in the systematic description and tabulation of the facts, by an increase in the number of the species." "When species are studied historically, the law appears evident that the characters of specific value . . . . present a greater degree of range of variability at an early stage in the life-period of the genus than in the later stages of that period." So marked is this incoming of new types in many cases that some students have supposed that actual special creation of species has occurred at these epochs. It should be said that there is apt to be a fallacy in observation in these instances, because the records which are, to our vision, simultaneous in the rocks may have extended over ages of time; but it is nevertheless true that some important groups seem to have come in somewhat quickly with

many or several species and to have passed out with exceeding slowness.

To my mind, all this is but the normal result of the divergence of character, or the survival of the unlike. A new type finds places of least conflict, it spreads rapidly and widely, and thereby varies immensely. It is a generalized type, and therefore adapts itself at once to many and changing conditions. A virile plant is introduced into a country in which the same or similar plants are unknown, and immediately it finds its opportunity and becomes a weed, by which we mean that it spreads and thrives everywhere. Darwin and Gray long ago elucidated this fact. The trilobites, spirifers, conifers, ginkgos, were weed-types of their time, the same as the composites are to-day. They were stronger than their contemporaries, the same as our own weeds are stronger than the cultivated plants with which they grow. After a time, the new types outran their opportunity, the remorseless struggle for existence tightened in upon them, the intermediate unlikenesses had been blotted out, and finally only one or two types remained, struggling on through the ages, but doomed to perish with the continuing changes of the earth. They became specialized and inelastic; and the highly specialized is necessarily doomed to extinction. Such remnants of a vanquished host remain to us in our single liriodendron, the single ginkgo and sassafras, and the depleted ranks of the conifers.

My attention was first called to this line of thought by contemplating upon the fact that cultivated plants differ widely in variability, and I was struck by the fact that many of our most inextricably variable groups—as the cucurbits, maize, citrus and the great tribes of composites—are still unknown in a fossil state, presumably because of their recent origin. Many other variable genera, to be sure, are well represented in fossil species, as roses (although these are as late as the Eocene), pyrus, prunus and musa; but absolute age is not so significant as the comparative age of the type, for types which originated very far back may be yet in the comparative youth of their development. The summary conclusions of a discussion of this subject were presented to the American Association for the Advancement of Science two years ago.* A modification of these points, as I now understand them, would run something as follows:

1. There is a wide difference in variability in cultivated plants. Some species vary enormously, and others very little.

2. This variability is not correlated with age of cultivation, degree of cultivation, or geographical distribution.

3. Variability of cultivated plants must be largely influenced and directed, therefore, by some antecedent causes.

4. The chief antecedent factor in directing this variability is probably the age of the type. New types, in geologic time, are polymorphous; old types are monomorphous and are tending towards extinction. The most flexible types of cultivated plants are such as have probably not yet

* *Proc. A.A.A.S.* 1894, 255; *Botanical Gazette*, xix. 381.

passed their zenith, as the cucurbits, composites, begonias and the like. The varieties of cereals, which are old types, are so much alike that expert knowledge is needed to distinguish them.

5. New types are more variable and flexible because less perfectly moulded into and adjusted to the circumstances of life than the old types are. They have not yet reached the limits of their dissemination and variation. They are generalized forms.

The reader will please observe that I have here regarded the origin and survival of the unlike in the plant creation in the sense of a plastic material which is acted upon by every external stimulus and which must necessarily vary from the very force of its acquired power of growth, and the unlikenesses are preserved because they are unlike. I have no sympathy with the too prevalent idea that all the attributes of plants are direct adaptations or that they are developed as mere protections from environment and associates. There is a type of popular writings which attempts to evolve many of the forms of plants as a mere protection from assumed enemies. Perhaps the plant features which have been most abused in this manner, are the spines, prickles and the like, and the presence of acrid or poisonous qualities. As a sample of this type of writing, I will make an extract from Massee's *Plant World*:

"Amongst the most prominent and general modes of protection of vegetative parts against the attacks of living enemies may be mentioned *prickles*, as in roses and brambles, which may either be straight, and thus prevent the nibblings of animals, or, in more advanced species, curved, thus enabling the weak stem to climb and carry its leaves out of harm's way. *Spines*, that are sharp-pointed abortive branches, serving the same purpose as prickles, as in the common sloe or blackthorn (*Prunus spinosa*). *Rigid hairs* on leaves and stem, as in the borage (*Borago officinalis*), and comfrey (*Symphytum officinale*). *Stinging hairs*, as in the common nettles (*Urtica dioica*, and *U. urens*). In these cases the stinging hairs are mixed on the leaves and stem with ordinary rigid hairs, of which they are higher developments, distinguished by the lower or basal swollen portion of the hair containing an irritating liquid that is ejected when the tip of the hair is broken off. *Bitter taste*, often accompanied by a strong scent, as in wormwood (*Artemisia vulgaris*), chamomile (*Anthemis nobilis*), and the leaves and fruit of the walnut (*Juglans regia*). *Poisonous alkaloids*, as in the species of *Strychnos*, which contain two very poisonous alkaloids, strychnine and brucine, in the root and the seeds; decoctions of species of Strychnos are used by the Javanese and the natives of South America to poison their arrows. Some of the species, as *Strychnos nux-vomica*, are valuable medicines, depending on the strychnine they contain, which acts as a powerful excitant of the spinal cord and nerves; thus the most effective protective arrangements evolved by plants can be turned to account, and consequently lead to the destruction of the individuals they were designed to protect. Our common arum (*Arum maculatum*), popularly known as 'Lords and Ladies,' has an intensely acrid sub-

stance present in the leaves, which effectually protects it from the attacks of mammals and caterpillars, but not from the attacks of parasitic fungi, which appear to be indifferent to all protective contrivances exhibited by plants, nearly every plant supporting one or more of these minute pests, the effects of which will be realized by mentioning the potato disease, 'rust' and 'smut' in the various cereals, and the hop disease, all due to parasitic fungi."

Now, this is merely a gratuitous and *ad captandrum* species of argument, one which is designed to please the fancy and to satisfy those superficial spirits who are still determined to read the element of design into organic nature. It does not account for the facts. These particular attributes of plants are specialized features, and it is always unsafe to generalize upon specializations. Each and every one of such specialized features must be investigated for itself. Probably the greater number of spinous processes will be found to be the *residua* following the contraction of the plant body; others are no doubt mere correlatives of the evolution of other attributes; and some may be the eruptions of the growth-force; and the acrid and poisonous properties are quite as likely to be wholly secondary and useless features. The attempt to find a definite immediate use and office for every attribute in the creation is superficial and pernicious. There are many attributes of organisms which are not only useless, but positively dangerous to the possessor, and they can be understood only as one studies them in connection with the long and eventful history of the line of ascent.

The thought which I want to leave with you, therefore, is that unlikenesses are the greatest facts in the organic creation. These unlikenesses in plants are (1) the expressions of the ever-changing environmental conditions in which plants grow, and of the incidental stimuli to which they are exposed; (2) the result of the force of mere growth; (3) the outcome of sexual mixing. They survive because they are unlike, and thereby enter fields of least competition. The possibility of the entire tragic evolution lay in the plasticity of the original life-plasma. The plastic creation has grown into its own needs day by day and age by age, and it is now just what it has been obliged to be. It could have been nothing else.

Remarks by Prof L. H. Bailey.

Prof. Cope has given us three general proofs or series of proofs of evolution. In the first place he says there is variation; in the second place succession; and in the third place we have the proof of embryology. I might subdivide them and might add two or three more proofs which appeal to me with particular force. It seems to me that we must accept the truth of evolution on the mere fact that the earth from its beginning has undergone wonderful physical changes, affecting the organisms living upon it, and which must have adapted themselves to the changes by them-

selves changing. In the second place, we know that there must be an intense struggle for existence amongst all forms of life; that the result of this struggle for existence must be adaptation to the organic environment. Again, another line of proof that evolution is true is the classificatory verification. The very fragment of the tree of life which Prof. Cope has put upon the board is an evidence that there are converging histories of animals, or, in other words, that there are relationships. But the proof which appeals to me most strongly is the fact that gardeners and breeders have it in their power to make new forms and that they have been making them since man began to deal with plants and animals. The palæontological and embryological records do not appeal to me with such force as the experiences of breeders and gardeners, who for ages have been modifying plants and animals almost to suit their will. This, of course, suggests that I am not skilled in the sciences of palæontology and embryology; but have given more attention to gardening.

I assume that you all believe in evolution. Heredity is not a necessary attribute of the theories of evolution. It is a matter for the physiologists and the embryologists to discuss rather than for one who looks broadly at nature and tries to discover some of the general and fundamental facts which have determined the onward progress of creation. I wish to call your attention to the facts of the origin of differences. I speak of differences rather than of variations.

Dr. D. G. Brinton made the following remarks:

We have listened with interest to this able exposition of the principles of evolution from three eminent scholars approaching it from different points of view. The question proposed, however, was one which was intended to go beyond the mere facts of natural science. Facts are not factors. The word means something more, something deeper. When we have these series of facts laid before us, however interesting they may be, they do not themselves express the primary law of evolution, but are merely a number of incidents illustrative of it. Therefore I think that the first speaker in his clear descriptions of the palæontologic evolutionary claims gave us little information as to the factors which brought them about.

We shall no doubt grant, as was urged by the second speaker, that there are extrinsic and intrinsic factors of evolution; but what he advanced as extrinsic factors were again series of external facts, and his intrinsic factors were series of internal facts or processes. The law by virtue of which they acted upon organic forms so as to produce a varying morphology was not, it seems to me, definitely stated.

By the third speaker the doctrine of evolution has been put forward as a sort of religious dogma of the scientific church. For myself, I cannot look upon it in that light. I believe I caught his words correctly when I quote him as saying that evolution holds good "from beginning to finish

of creation." I cannot see that any known facts justify such a statement. Evolution is a matter of the past not of the future. We have nothing to do with the "finish of creation," and it is not likely that we know anything about it. Such a dogma has no place in scientific bodies. All we know is, that of the many millions of organized species a few have developed into higher forms, while the immense majority have perished utterly. We have no guarantee but that evolution has reached its acme and may cease to-night. Let us hold it, therefore, as a fact of past time, not as a dogma of faith regarding the future.

Turning now to the question of the evening, What are the ultimate factors or primary causes, so far as we can trace them, which have influenced and do influence the development of organic forms? For an answer I turn to an expression once used by my teacher, Prof. James D. Dana, whose name is a household word to every man of science. His suggestive expression was, "The whole of Nature is bound in a straight-jacket of mathematics." It means that we must go back to the purely mechanical forces of the universe, if we would find the primary factors of organic variation. The last speaker well said that mutability, change, not permanence, is the law of organic life. He developed it admirably in his references to the like and the unlike, and in his statement that unlikeness is really the secret of advance. This theory, as doubtless some will remember, was that brought forward with force and beauty by the late eminent Dr. Pasteur in his remarkable papers on *Asymmetry* as the source of change in both the organic and inorganic worlds. Unquestionably he was right. Change is the law of the universe. It is no new perception of the thinking mind. Nigh two thousand years ago the philosopher Heraclitus of Ephesus laid down the principle, "All is flowing," *παντα ρει*. No two organic forms are alike, or can be alike. The son is never the image of his father; the plant never finds in its product the precise reproduction of itself. You remember how Leibnitz amused the ladies of the court by having them try to find two leaves of an oak which were alike. They tried in vain. Never anywhere is uniformity or identity; everywhere is indefinite, infinite variability.

What is the explanation of this?

I ask your attention again to the mechanical principles of nature. To them alone must we return when we search for primary agencies of change. All organic and inorganic substances are constantly subject to the innumerable forces which play upon them from all parts of the universe. Every atom of earth is influenced by each distant star. Constantly each atom is bombarded by thousands, by millions of forces, and its changes are the resultants of these.

The primary laws of motion with which we are familiar in the *Principia* of Newton are also the primary causes both of the permanence and the variability of organic forms. His first law—that motion would continue forever in the same direction unless interfered with by other motion in

another direction—gives us the stability of species, the potent tendency of the individual to transmit the specific characteristics, the maintenance of traits by the germinal protoplasm, as brought out by the second speaker. It is the *conatus in se perseverare* of Spinoza.

The second law of motion is the basis of all change and variation It is, as doubtless you remember, that change of motion is proportional to force and takes place in the line of the force. Infinite forces infinitely different in power are forever acting on every atom, and its changes are the resultants of them all.

These ceaseless changes are purely mechanical, and mechanical laws produce their results absolutely without regard to future aims, absolutely indifferent to the quality of results, whether towards evolution or degeneration. For that reason. I repeat that any dogmatic assumption of evolution as a law of nature is unscientific. Of a million changes, a few may act in so strengthening the energy of the primary and permanent characters that they will resist the deterrent or subversive action of other forces So far as we know, this is mere chance. Purely mechanical forces decide the progress of a species or its extinction. Beyond such mechanical, mathematical laws, natural science has no right to go.

In conclusion, I would say a few words in reference to "sports," a topic introduced by the last speaker. These sudden and extensive changes received the careful attention of Darwin, who in his work on the *Domestication of Animals and Plants*, refers to it by the term "spontaneous variation . . . ." He pointed out that in some cases it is extraordinarily great and also permanent, as in the instance of the niata cattle in La Plata. In the vegetable world, Mr. Meehan has illustrated this form of change by numerous and striking examples. The last speaker mentioned that the lines of species had not been traced through sports. I would call attention to the obvious fact that the origin of what are called specific peculiarities from a sport would be likely to cause the scientific investigator to lose the trail at that point. Darwin says that nothing but the record would reconcile us to believing that such sports as some he describes issued from the species to which they belong.

How unconsidered then is the remark of the last speaker in reference to those who have suggested that man himself may have owed his specific peculiarity to such an origin! There is nothing impossible in this, nothing incredible, nothing absurd. When our ancestors ascended from the plane of the beast to that of reasoning intelligence, a part of the path may have been won by one of those bounds which have been called saltatory evolution. There is nothing in this contrary to either theory or observation. It is supported by both; and having once gained that higher plane, they would not willingly have forfeited its advantages.

Further remarks by Prof. L. H. Bailey:

Dr. Brinton has quoted me as saying, "From beginning to finish of creation, evolution is true." He quoted me correctly. That is my own

conviction. I have no proof. I have no proof that the sun will rise to-morrow. But the greater the collection of facts and of data which we make respecting the evolution of the world in the past, the more are the changes seen to be continuous and gradual; and it seems to me that if evolution has taught us anything it has been to show that there is a law of evolution, continuous throughout time. I believe, myself, that evolution is true from beginning to finish of creation; and if we could not prophesy that our race has nobler possibilities for the future I should lose my zest to live.

Spontaneous variations are not necessarily sports in the sense in which I refer to them. Sports are those forms of variation which appear to lie outside the general or customary type of variation of the species—or phylum—with which we are dealing. They are those forms which are so unusual as to be ordinarily considered to be a taxonomic variety or division or subspecies. The causes of sports are unknown to us, as are also the causes of all spontaneous differences which may be of much less moment. The fact that Darwin dwelt upon the origin of sports in domestic animals is a matter which I discussed in my paper and, I believe, it is the chief line of effort in which man's work differs from nature's—the fact that he does save the sports and breed them up. I have no evidence that nature does the same; and so far as the plant creation is concerned, I am more and more convinced that sports have had but comparatively small influence upon the phylogenies of our present types.

I wish to add just one word in reference to a matter which Prof. Conklin introduced. He took issue with Prof. Cope with respect to the doctrine of natural selection and the notion that Darwin did not attempt to account for variation. The doctrine of natural selection itself does not account for variation. It has been the misfortune of Darwin's writings that his doctrine of natural selection has been so emphasized as to overshadow everything else which he did. Amongst the causes of variability which Darwin enumerates are external stimuli, soil, weather, food, climate and other impinging factors; so that Darwin conceived the idea that impinging stimuli were the causes of variations which, when they have arisen, have been bred up by natural selection.